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Isaac Cassel Davis

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE

AND THE ASSOCIATE SCIENCES.

EDITED BY

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VOL. III.

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Contents of No. IV, Vol. III.

ORIGINAL COMMUNICATIONS.

ART. I. Thoughts on Febrile Miasms, intended as an answer to the Boylston medical prize question, for the year 1830, "Whether Fever is produced by the decomposition of animal or vegetable substances, and if by both, their comparative influence?" By CHARLES CALDWELL M. D. 457

II. An Essay on the Absorbent System and its Functions. By JAMES CONQUEST CROSS M. D. of Fayette county, Kentucky. (Continued from page 364.) 520

III. An Essay on the Origin of the Plague. By JOHN ESTEN COOKE M. D. &c. 553

IV. Case of Phlegmasia Dolens successfully treated by the application of the Bandage. Communicated in a letter from DR. WILLIAM T. YOUNG, of Triana, Alabama, to PROFESSOR DUDLEY. 569

V. Case of Gun-shot wound of the elbow-joint, treated by the Bandage. By DR. JACOB S. SWAN, of Henderson county, Kentucky. 571

MISCELLANEOUS INTELLIGENCE.

1. Observations on the blood. By William Steevens, M. D. 572

2. On the production of Worms in the human body. By J. Scouler, M. D. &c. Professor of Natural History in the Andersonian University. 577

3. On Amaurosis. By James J. Knox, M. D.—Amaurosis from Suppressed Catamenia. 581

4. Amaurosis from sympathy. 582

5. Cursory Remarks on the Presence of Worms in the Large Bowels. By James Annesley, Esq. 583

6. Case in which Lumbrici passed from an opening at the Umbilicus. 584

7. Madame Boivin on a new cause of Miscarriage. 585

8. Remarks on the Treatment of Amaurosis by Strychnine. By Thomas Shortt, M. D. &c. 588

9. On Oleum Terebinthinæ as a remedy for Salivation. By E. Geddings M. D. &c. 159

NOTICES.

Since the publication of our last number we have received,

1. The American Medical Journal of the Medical Sciences, Nos. XII, XIII, quarterly. (In Exchange.)
2. The North American Medical and Surgical Journal, No. XX, October 1830, quarterly. (In Exchange.)
3. The American Journal of Science and Arts, conducted by Benjamin Silliman M. D. L. L. D. &c. No. I, Vol. XIX, quarterly. (In Exchange.)
4. The Western Journal of the Medical and Physical Sciences, No. III, vol. IV, quarterly. (In Exchange.)
5. The Journal of Health, conducted by an Association of Physicians in Philadelphia, twice a month; Nos. 23, vol. I to No. 5, vol. II, inclusive. (In Exchange.)
6. The Journal of Law, conducted by an Association of Members of the Bar in Philadelphia, twice a month; Nos. 3 to 10, inclusive. (In Exchange.)
7. The American Lancet, semi-monthly: Nos. 3 to 11, vol. II. (In Exchange.)
8. The Southern Review, No. XII, November, 1830, quarterly. (In Exchange.)
9. The Illinois Monthly Magazine, conducted by James Hall. Nos. 1, 2, 3, vol. I.
10. The Boston Medical and Surgical Journal, weekly. No. 33, vol. III. (In Exchange.)
11. An Essay on the comparative influence of Vegetable and Animal Decomposition as a cause of Fevers. By Usher Parsons M. D. Professor of Anatomy and Surgery, Providence R. I. (From the Author.)

We are requested by Professor Caldwell to publish the following order of the Boylston prize committee, of which he was ignorant at the time his Essay was printed in the present Number of our Journal.

By an order adopted in the year 1826, the Secretary was directed to publish annually the following votes, viz.

1st. That the Board do not consider themselves as approving the doctrines contained in any of the dissertations to which the premiums may be adjudged.

2nd. That in case of the publication of a successful dissertation, the author be considered as bound to print the above vote in connexion therewith.

GEO. HAYWARD, *Secretary*.

Boston, August 6th, 1830.

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE,

AND THE ASSOCIATE SCIENCES.

FEBRUARY, 1830.

ORIGINAL COMMUNICATIONS.

ARTICLE. I.—*An Essay on the History, Causes and Treatment of Typhus Fever;* to which the Annual Prize for the year 1828, was awarded by the Medical Society of the State of New York. By ALFRED T. MAGILL, M. D. of Winchester, Virginia.

(CONCLUDED FROM VOL. 2, p. 577.)

"Ex principiis nascitur probabilitas; ex factis vero veritas."

BLISTERS.—Much variety of opinion exists as to the utility of blisters in fever. While some recommend them early in the disease,* others confine their application to its advanced stage, and some there are who reject them in toto;† and there are

* Lind remarks, that "In a moderate infectious fever, where the source of infection is not very violent, if twenty patients be blistered, sixteen will next morning be entirely free from headache, heat, pain, and fever."

† Dr. Moore, in his "Medical Sketches," observes, that "Notwithstanding my having watched the effect of blisters with all the attention I am capable of, and formerly with a strong prepossession in their favor, I cannot assert that I ever knew vesications of any use in this disease, (i. e typhus;) but I have frequently seen the patient teazed by their irritating quality, without their seeming to have any other effect."

others still, who think them only beneficial when applied to relieve some topical affection. We are among the latter number. In cases of fever attended with inflammation of some important part, we have often witnessed the most remarkably good effects from their use, and never fail to apply them under such circumstances. We entirely agree with those who deprecate their early application in continued fever, not characterized by some local affection, aud even in such cases we should always prefer using free depletion before resorting to them. When applied without the presence of inflammation, they are decidedly injurious; they increase the fever and irritation of the patient, and often prove a source of much torment to him. The language of Fordyce on this subject is exceedingly judicious, and ought to have great weight with all those who believe that experience is the best teacher, particularly in matters relating to medicine. Fordyce was probably as conversant with fever as any man of his day, and therefore when he speaks we should listen with attention. He says, that "Whether exciting inflammation has or has not the same effect in a regular continued fever, which it has in health, can only be known by making the application to the body of a person affected with regular continued fever. As far as the author's experience goes, when any stimulus has been employed so as to produce inflammation, when the patient has become weak towards the end of a continued fever, the only difference which has occurred has been, that phlegmonous inflammation has not produced hardness, fullness, and strength of the pulse, but both phlegmonous inflammation and inflammation of the skin have occasioned greater frequency of the pulse, have rendered it weaker and smaller, and as in health have prevented sleep, and the patient's taking the same quantity of nourishment, and have depressed and deranged the whole system."* Huxham, an old author, and on this account many may think his opinions not entitled to much weight, though we are free to confess that we think not the less for that reason, has some excellent remarks on this subject. "What I have said (observes he) of volatile

* Fordyce on Fever, p. 270.

alcalious salts, leads to a reflection on the promiscuous use of blisters in these fevers, which by some are deemed the only *anchor of hope* in such dangerous cases; but I think they are many times too hastily and improperly applied, especially in the beginning, when the fever runs high, and doth not demand a farther *stimulant*; for the action of cantharides is not confined to the skin, but affects the whole nervous and vascular system: now when the irritations and vibrations are already too great, as frequently happens in the beginning of such fevers, they are very injudiciously applied. It is true, indeed, (continues he,) nature may sometimes want a *spur*, nay, often doth so, particularly towards the decline of these fevers, when the solids grow torpid, the circulation languid, the spirits effete, and the sick comatose; but in the above circumstance I have very many times seen very pernicious effects attend their too early application, as obstinate *pervigilium*, *delirium*, suppression of urine, *tremors*, *subsultus*, &c."* We find from this extract, that though Huxham disapproves of the use of blisters in the early stage of fever, yet he acknowledges the occasional necessity for their use in the last; yet even under the circumstances he has mentioned, we should greatly prefer the use of sinapisms, because they are much more powerful and more immediate in their effect, and are not so apt to produce a troublesome and tormenting sore; this last effect we have often known to occasion much fretfulness and pain to the patient during the whole stage of convalescence. Another objection to the use of blisters in the last stages of this fever, when the debility is extreme, is the probability of their producing gangrene in the part to which they are applied. Sir John Pringle, Clark, and Lind, and many others, bear testimony to this effect of blisters, when applied to patients under such circumstances.

From the opinions of the distinguished physicians which we have quoted, and from our own experience, we are justified in the conclusion that blisters are inapplicable in the early stage of fever, and that they are rather of equivocal benefit when

* Essay on Malignant Putrid Fevers.

applied in its advanced stages; used, however, at any stage to relieve topical inflammation, they are of eminent service; they always do most good, however, when resorted to after the pulse has been reduced by proper depletion, and this, indeed, ought always to be done.

When blisters are used to relieve local inflammation, they should be applied immediately over the part affected, or if this be impracticable, then as near to it as possible. Blisters frequently produce a very unpleasant affection of the bladder, called strangury; this can be most generally relieved by drinking freely of flax seed tea, barley water, &c.; if this should fail, we can recommend, as an infallible remedy, the injection of five, ten, or twenty grains of camphor, suspended in about a gill of mucilage of gum arabic.

Bark and Wine. These two remedies were formerly much relied on for the cure of typhus fever, and even at the present day there are many who adhere to this mode of treating the disease. It is, however, one of the relics of exploded error which will soon cease to exist, and is only to be found in those who are fettered by educational prejudices, which they are unwilling or unable to break. If this disease be sthenic in its character, then it necessarily follows, that tonics and stimulants are not the proper remedies for its cure. We believe that bark is never necessary in this fever, unless when it changes to an intermittent type, and here it may be given with excellent effect. We suspect strongly with Dr. Rush that when bark cures this or any other disease of excitement, it does so by acting as a purge; for we know that when given in any quantity it very frequently acts in that way. Bruce says that bark cured the bilious fevers of Massuah when it operated as a purge. Stimulants we reject not in toto, but almost entirely in the cure of fever we feel the most unrelenting prejudice against them; for it has fallen to our lot to see many cases in which irreparable injury was produced by their improper and unreasonable exhibition.* We

* Stimulants are injurious in proportion to the violence of the fever, and hence they are so destructive in the high grades of the autumnal fevers of this country. Rush deprecated, with all his earnestness and enthusiasm their exhibition in yellow fever, and some of the older authors also mention their fatal

have frequently known patients stimulated into a delirium, and sometimes into apoplexy; and we have known them rescued from the latter condition by the supervention of a free bilious diarrhœa, a most convincing proof that the supposed debility of the patient, to counteract which stimulants were resorted to, did not demand such treatment. It is that bugbear debility which frightens us into too early a resort to remedies of such pernicious tendency; and until we can divest ourselves of the fears arising from this delusive source, we will often do wrong by administering them at an improper period of the disease.

However strongly we may be opposed to stimulants as constituting a necessary part of the general treatment of fevers, we yet think them highly important in their proper place, and of indispensible utility under certain circumstances. When a patient is very much prostrated from the long continuance of fever, we may often unite, with great advantage, some stimulant to the means used for subduing the fever. Camphor and wine are the two most frequently resorted to. We greatly prefer the latter to any other kind of stimulant; the camphor, when used in any quantity, almost always producing a most unpleasant delirium. Of the different wines, Madeira is much the best. Armstrong says, that in the advanced stages of typhus he is in the habit of mixing milk with the wine, and under this form he thinks it makes an excellent drink.

It will be seen by the above remarks, that we are decidedly opposed to stimulants, in every stage of the fever, except under the circumstances mentioned. The majority of cases of typhus, when treated properly in the primary stages, will not often require the use of stimulants in the last. Hamilton, some years after he commenced the purgative plan of treatment, says, "I cannot, however, omit remarking, that for some years past I find wine less necessary in fevers than I formerly thought it was;" and every unprejudiced physician, who properly em-

effects in fevers. Thus Shenkins, in speaking of the famous Hungarian fever, which prevailed so extensively in Europe about the middle of the sixteenth century, says, "omnes qui vini potione non abstinuerunt, interiere; adeo ut summa spes salvationis in vini abstinentia collocata videtur."

ploys depletory measures in the beginning of fever, will acknowledge that stimulants are but rarely necessary in the last stage.

Of the Diet and Drinks. We have chosen to consider the subject of regimen under a separate head, because we think that a proper regulation of it has considerable influence over the state and progress of the disease. From much attention to this subject we are convinced that fever is often kept up and protracted by improper articles of diet. The friends of the patient usually betray such excessive anxiety upon this subject, are so much afraid of his dying from mere inanition, and are generally so importunate in their intreaties for more nourishment, and so much dissatisfied with a refusal, that few physicians have fortitude enough to enforce the dietetic plan as rigidly as they could wish, and as would best comport with the interests of their patients. The rigid enforcement of a very low diet in fever is, we think, the only respect in which Broussais' practice ought to be followed.

The ancient physicians were well aware of the great importance of a suitable diet in fever, and accordingly paid much attention to it. Hippocrates permitted the patient to drink freely of cooling beverages, and gave but little nourishment during the first days of the disease. Erasistratus, who lived about two hundred years after Hippocrates, being of opinion that fever proceeded from too great plethora, enjoined a general and rigid abstinence upon his patients. Asclepiades and his followers also prescribed abstinence, but limited its duration to three days; he permitted the patient neither to eat nor drink during this time, and says that in this way the fever was cured *tuto celeriter et jucunde.* The practice of limiting the abstinence strictly to three days was distinguished by the name of diatrition, and those who followed it by the appellation of diatritarii. We find, therefore, from what has been mentioned, that the ancient physicians thought a proper diet a matter of considerable importance in fever.

The regimen during the fever should be of the lightest and most abstemious character. We do not think that we can well

err by the utmost strictness on this point; we should literally starve the fever. Animal food should be proscribed as pernicious in the extreme. A cup of barley water, or of weak tea or coffee, twice a day, with a hard water cracker, if the patient should desire it, is generally sufficient for him; he should also be allowed to drink freely of cooling beverages, such as lemonade, apple water, rice water, currant jelly water, toast and water, balm tea, and cold water; here is variety enough to satisfy the most wayward and capricious appetite, and which can be indulged in freely without the fear of injury.

But it is not during the febrile state alone, that we should be particular about the diet of the patient; much attention to it is also necessary during the stage of convalescence. The patient should be permitted to return gradually to his former mode of living, commencing with the lightest and most digestible articles of diet, such as arrow root, sago, tapioca, &c. Animal food ought not to be allowed until he is able to walk about; a too early use of the latter is often the cause of severe relapses, and we are earnestly cautioned against it by most writers on fevers. Thus Huxham, in his account of the malignant fever which prevailed at Plymouth in the year 1740, remarks, that "If any one made use of a flesh or a fish diet, before he had been very well purged, and his recovery confirmed, he infallibly indulged himself herein at the utmost danger of his life;" and Dr. Poissonier, in his work on the maladies of seamen, says, that "the physicians of Brest have observed, that the relapses in the malignant fevers which prevailed in their naval hospitals, were as much the effect of a fault in the diet of the sick, as of the contagious air to which they were exposed, and that as many patients perished from this cause as from the original fever. For this reason light soups, with leguminous vegetables in them, panada, rice, seasoned with cinnamon, fresh eggs, &c., are all they should be permitted to eat. The use of flesh should be forbidden for many days after the entire cure of the disorder."* In addition to the articles mentioned in the latter

* Maladies de Gens de Mer. vol. 1, p. 345.

part of this quotation, the patient may be allowed to eat moderately of ripe fruit; the pulp of roasted apples is very good; milk and bread may also be allowed him. There is one circumstance, however, which ought always to be attended to during convalescence, which is to keep the bowels open; if this does not happen spontaneously, it must be effected by the use of laxatives; this is highly important, as it lessens greatly the danger of a relapse. Armstrong says, that he has seldom seen relapses in inflammatory diseases, when the bowels have been kept regular, and the antiphlogistic diet observed.

We have now finished what we have to say on the treatment of this form of typhus, and, from a review of it, it will appear, that all the remedies mentioned concur in the same tendency, i. e. to reduce the action of the heart and arteries, and to remove local inflammation when present. This course of treatment is the necessary and unavoidable result of the opinions we have adopted in relation to the pathology of typhus; if these are correct, if the disease is really one of excitement, and often of inflammation of some vital organ, then it follows, that the antiphlogistic plan of treatment, differently modified according to the grade of the fever, is the proper one. Whether, however, the views of this disease, inculcated by the great Sydenham, proclaimed and defended in modern times by Armstrong and others, and embraced by the humble author of this essay, be true or not, time and the increasing light of the science must decide. The great object in our profession certainly is, to ascertain the true character of diseases. If our pathological views are correct, then the practice legitimately deducible therefrom must be correct. It is one of the maxims of the great father of medicine, that he who knows the nature of a disease knows how to cure it, or, as Celsus elegantly expresses it, "Estimatio causæ sæpe solvit morbum;" and all our endeavors should be directed to this point. Until this be accomplished, our practice must partake somewhat of empiricism.

Treatment of Congestive Typhus.—It has been already mentioned, that in this form of fever, the system does not react from the first stage of oppression, but continues in this state, in a

greater or less degree, according to the violence of the cause producing it. In some cases the prostration is complete; in others, there is a partial and transitory reaction. From the appearances of great debility which present themselves in the commencement of congestive typhus, it has been customary to treat it by the administration of cordials and stimulants. This treatment would be proper enough if the debility was real; but if it is only apparent, if it be the result of that change in the circulation which has been noticed in a former part of this essay, and that (in the language of Armstrong) "it is no more to be accounted positive exhaustion than the loss of muscular power, which precedes and accompanies the treating of genuine apoplexy, to which, in fact, this modification of typhus has often a most forcible resemblance;" then it demands a different treatment. If, then, cordials and stimulants are not to be relied on for the cure of this form of typhus, (though we acknowledge that they are occasionally very useful adjuvants to the other means,) what is the proper plan of treatment? We answer, that our first endeavour should be to remove some portion of that blood, which is, (if we may use the expression,) suffocating the heart's action, by abstracting blood from the system. Under such circumstances we frequently succeed in relieving the heart, in some measure, of its overwhelming load of blood, and, as a necessary consequence of this, raising the pulse: this appears very paradoxical, but it is nevertheless true. Sydenham gives a remarkable instance of this effect of bleeding upon the pulse. In the case of a young man, whose skin was so cool that he could not persuade the attendants he had a fever, and who appeared to be almost expiring, he drew blood freely, with the effect of raising the pulse to such a degree, that it required three or four subsequent bleedings to subdue it: bleeding, however, does not always have this effect; it sometimes weakens instead of raising the pulse. Its producing or not this latter effect is the test of its utility. If the pulse becomes more free and full under the loss of blood, then we may be certain that it is doing good; if, on the other hand, it renders it more feeble and weak, we may be equally assured that it is doing

injury. We should therefore always keep our finger on the pulse, and be guided in the quantity to be drawn, by its effect on the beat of the artery: if it should become more full and expanded, we may suffer it to flow; if the reverse, it should be arrested immediately. It is often, however, extremely difficult to draw blood in congestive fever; "the acting of the heart (says Armstrong) is often so much overpowered in the first instance, that the blood merely trickles, or rather oozes, from the punctured vessel, for a considerable time, being much darker and thicker than natural; yet when a few ounces have been drawn, it usually flows with freedom, and becomes finally of a brighter colour. Occasionally I have stood over a patient nearly half an hour before a stream of blood could be obtained from a vein, but at last it gushed out in a full current, and was not as easily restrained as in ordinary cases. Many a life (continues he) might be lost by binding up the arm too hastily, and, therefore, the operator should persevere, until he knows whether enough blood can be obtained." If we cannot, however, procure blood from a vein, we must resort to arteriotomy.

The manner of drawing the blood is a matter of some importance; it should not be abstracted suddenly and by a large orifice, but gradually;* by proceeding in this way the heart much more certainly recovers its power. When the action of the heart and arteries has become more free, we ought then to arrest the flow of blood, as any further loss might again render this action weak, and place the patient in a worse situation than

* Dr. Rush forcibly illustrates the propriety of gradually abstracting blood, in certain states of malignant fever, by comparing the system to "a man struggling beneath a load of two hundred weight, who is able to lift but one hundred and seventy-five. In order to assist him (says Rush) it will be to no purpose to attempt to infuse additional vigor into his muscles by the use of a whip, or of strong drink. Every exertion will serve only to waste his strength. In this situation (supposing it impossible to divide the weight which confines him to the ground) let the pockets of this man be emptied of their contents, and let him be stripped of so much of his clothing as to reduce his weight five and twenty or thirty pounds. In this situation he will rise from the ground; but if the weights be abstracted suddenly, while he is in the act of exertion, he will rise with a spring that will endanger a second fall, and probably produce a temporary convulsion in his system. By abstracting the weights from his body more gradually, he will rise by degrees from the ground, and the system will accommodate itself in such a manner to the diminution of its pressure, as to resume its erect form, &c."

he was before the bleeding. "When both the pulse and the general circulation become manifestly free, (after bleeding,) with a warm moist skin, tepid diluents should be the only beverage recommended; yet, when it still remains oppressed, and the tide of the circulation does not return to the surface, and more especially if blood has been freely drawn, some wine with warm water should be occasionally exhibited, and the patient speedily immersed in a bath strongly impregnated with salt, and at least about the temperature of 100° Fahrenheit's scale. He should remain in the bath till his skin becomes warm, and, on being removed, it should be well rubbed all over with hot flannels, and he ought then to be laid in an aired bed, with bottles of warm water at his feet. This plan, together with tepid wine and water occasionally, will often promote a flow of blood towards the skin, and relieve the viscera from congestion."*

But we are not to rely solely on bleeding, the bath, &c. in the cure of congestive fever. After bleeding we should immediately order large stimulating enemata, that the bowels may, if possible, be at once evacuated; and then commence the use of cathartics. Among them calomel is beyond all comparison the best; this given alone, or in combination with jalap, in doses sufficient to act freely on the bowels, will often prove of immense utility. One of the first triumphs of the calomel and jalap practice, instituted by the great Rush in the yellow fever of 1793, was the recovery of an apparently hopeless and desperate case of congestive fever. "The patient (says he) had passed twelve hours, before, I began to give him this medicine, without a pulse, and with a cold sweat on all his limbs. His relations had given him over, and one of his neighbours complained to me of my neglecting to advise them to make immediate preparation for his funeral. But in this situation I did not despair of his recovery. Dr. Mitchell's account of the effect of purging in raising the pulse, excited a hope that he might be saved, provided his bowels could be opened. I now committed the exhibition of the purging medicine to Mr. Stall,

* Armstrong on Typhus.

one of my pupils, who mixed it and gave it with his own hands, three or four times a day. At length it operated and produced two copious fetid stools. His pulse rose immediately afterwards, and an universal moisture on his skin succeeded the cold sweat on his limbs. In a few days (continues he) he was out of danger, and soon afterwards appeared in the streets in good health, as the first fruits of the efficacy of mercurial purges in yellow fever." We have copied the whole of this case, because of its very instructive and interesting character. It evinces in a striking degree the effect evacuations from the bowels have in raising the pulse, and it should also teach that no case, however bad in appearance, should be abandoned as incurable: our motto indeed should be *nil desperandum.*

The bowels, in congestive fever, are often extremely torpid, and require the most powerful doses to rouse them to action. This is one of the cases in which we should disregard the injunctions of our materia medicas, and exhibit medicine in doses commensurate with the violence and obstinacy of the disease.

Armstrong has great confidence in the salivatory effects of calomel. We must confess that we believe it never does good in this way; the calomel, before it affects the mouth, has already won the victory over the disease. The reason why salivation has been thought to put an end to fever, is the generally simultaneous appearance af ptyalism and the cessation of the fever; but we are fully convinced that the former is the result of the latter, and not that the latter is produced by the former. Salivation, therefore, may generally be considered as a good sign, as it is a proof that the fever is either entirely subdued or very much on the wane.

Our great confidence, therefore, in calomel is from its superior efficacy in evacuating dark or green matter from the bowels; and in this only we believe consists its beneficial effect in fever.

e only local remedy which we have found very useful in of congestive fever, is the application of mustard poulti-
c apisms to the wrists, ankles, and pit of the stomach.
surface of the body is cool and the pulse very weak

and low, these sometimes produce an excellent effect; they are, particularly in such cases, greatly preferable to blisters.

Causes of Typhus Fever. We come now to a consideration of the causes of typhus. This is a subject on which great diversity of opinion exists even at the present day. While some are of opinion that it may arise from the putrefaction of animal matter, or from human effluvia, the result of crowding healthy persons into confined and ill ventilated places; others contend that it arises exclusively from, and is propagated by, a specific contagion; and there are others still who deny to it any such property, and consider it merely as a variety of the common remittent and continued fever, and, of course, as arising from the same causes.

We shall examine these different opinions in succession, with as much fairness and impartiality as we can; and first, of that opinion which attributes the origin of typhus fever to animal putrefaction. This has for a long time been considered not only as a cause of the fever now under consideration, but of other fevers also. Pringle remarks, that "The hospital and jail fever are to be considered as the same disease, and little, if at all, different from such as have arisen after battles, when the bodies of the slain have been left unburied to rot upon the field. This, Galen (continues Pringle) notes as one of the causes of pestilential fevers."* There is, however, such an abundance of evidence in refutation of this opinion, as "to leave no loop or hinge to hang a doubt on." Bancroft, in his able and philosophical work on fevers, has collected a mass of testimony to this effect, some of which we shall take the liberty to copy into this essay. The most remarkable fact which he relates, is the exhumation made in the churchyard of the Saints Innocens, at Paris, in the year 1786. "The churchyard of the Saints Innocens, at Paris, situated in one of the most populous quarters of the city, had been made the depository of so many bodies, that, although its area enclosed more than 1700 square toises, or near two acres, yet the soil had been raised by them

* Observations on Jail or Hospital Fever, p. 320.

eight or ten feet higher than the level of the adjoining streets; and upon the most moderate calculation, considerably more than six hundred thousand bodies had been buried in it, during the last six centuries; previous to which date, it was already a very ancient burial ground. Numerous complaints having been made concerning the offensive smells, which arose from this spot, and sometimes penetrated into the adjoining houses, and the public mind being greatly alarmed, it was at last determined to forbid all future burials there, and to remove so much of the superstratum as would reduce the surface to the level of the streets. This work was undertaken in 1786, under the superintendance of M. Thouret, a physician of eminence in Paris, and in two years he accomplished the removal of that superstratum, almost the whole of which was impregnated, or *infected*, as M. Thouret styles it, with the remains of carcases, and of quantities of filth and ordure, thrown upon it from the adjoining houses."

"The exhumations" (says M. Thouret) "were principally executed during the winter; but a considerable part of them was also carried on during the *greatest heats* of summer. They were begun with every possible care, and with every known precaution; but they were afterwards continued, almost for the *whole period* of the operations, without employing, it may be said, *any precaution whatever;* yet no danger manifested itself in the whole course of our labors—no accident occurred to disturb the public tranquility!"*

The philanthropic John Howard, in his work on Lazarettos, p. 25, remarks, that "The governor, at the French hospital at

* "It does not appear, after the fullest enquiry, that any febrile disorder was ever produced by this immense mass of corruption, during the removals made in 1786, or while it was suffered to remain as a burying ground. The grave diggers were, indeed, sometimes thrown down suddenly, and, for a time, deprived of sense and motion, by the concentrated vapors which escaped, upon accidentally breaking open, by their spades, the abdominal viscera of bodies, in an early stage of putrefaction. These vapours, also, in a more diffused state, are said to have produced nausea, loss of appetite, and in a course of years, paleness of countenance, debility, tremors, &c. But fever of any kind, and much less CONTAGIOUS FEVER, does not appear to have been noticed, as resulting from the offensive or putrid matters of this churchyard, either to the grave diggers or to the neighbouring inhabitants." Bancroft, pages 93, 94, 95.

Smyrna, told me that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent, especially if he opened any of those windows which looked toward the great burying ground, where numbers were left, every day, unburied; but that it had no effect on the health of himself or his family. An opulent merchant, in this city, adds he, likewise told me that he and his family had felt the same inconvenience, without any bad consequences."*

In the Edinburgh Medical and Surgical Journal, of October, 1810, is an account, given by Dr. Chisholm, of a manufactory at *Conham*, near Bristol, destined for the converson of animal flesh into a substance resembling spermaceti, by cutting up dead horses, asses, dogs &c. and putting their muscular parts into boxes with holes for the admission of water, and afterwards placing them in pits filled with water; while the entrails and useless parts of many hundreds of carcases, were left to putrefy on the surface of the ground. The effluvia of these putrefying animal matters were highly offensive to the overseer of this manufactory, and to the workmen employed under him, as well as to others in their reach, yet no injury was done to the health of any person during the two years in which these operations were continued."†

In addition to these strong facts, and many others equally so, which we deem unnecessary to mention, we may add that in no one instance that we have heard of, has typhus fever been attributed to the concentrated and putrid effluvia arising from the numerous dead bodies in the dissecting rooms attached to the different medical colleges in this and other countries. These facts are considered sufficient to prove that fever of no kind or description ever arises from animal putrefaction.

The next assertion, that typhus fever is produced by the effluvia arising from crowding healthy persons into close and ill ventilated places, is equally destitute of foundation. This is an almost universal opinion. We can hardly take up any work on the subject, in which we do not find this set down as one of

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*Bancroft, p. 97. †Bancroft, p. 442.

the causes of typhus. Cullen, Chisholm, Good, and Wilson, all mention human effluvia as one of the causes of this fever. Cullen remarks, that "effluvia constantly arising from the human body, if long retained in the same place without being diffused in the atmosphere, acquire a singular virulence; and in that state being applied to the bodies of men, they become the cause of a fever which is highly contagious." Chisholm says, "the cause, in fact, of *typhus*, is, I believe, an undefined change in the atmospheric air, brought about by its confinement in a very limited space, and incapacity, in a great degree, of renewal, and the respiration of an effluvium, emanating from the persons inhabiting the wretched close dwellings in which fever is found."* Good, when speaking of the causes of typhus, remarks, that "its common cause is febrile miasm, issuing from the decomposition of human effluvium, under the influence of the ordinary auxiliaries of a close and stagnant atmosphere."† Wilson, likewise, says, that "the effluvia of the living body, become putrid by stagnation, are capable of producing it,"‡ i. e. typhus fever. We see, therefore, that the ablest writers on medicine have adopted and published this opinion; and when bolstered up by such authority, it is not surprizing that it should have become so general. Bancroft, however, by a series of the most undoubted and convincing facts, has completely overturned this opinion, and has proved beyond cavil or doubt, that it is never productive of typhus fever. We will select the most striking of the facts recorded on this subject.

"The people of the island of Oonalaska inhabit jourts, or subterraneous dwellings, each common to many families, in which they live in horrible filthiness." (Pennant's Arctic Zoology, vol. 1.) "And the Samoiedes live in subterraneous dwellings, equally filthy, for almost nine months in the year, who are yet reported by travellers to be strong, active, and healthy. In addition to all this filth, crowding, and want of ventilation, the *food* of these people may be considered as little better than

* Bancroft, p. 99. † Study of Medicine, vol. 2, p. 124.
‡ On Fevers, vol. 1, p. 154.

putrefaction itself." Mr. Pennant, describing that of the natives of Kamschatka, says, "their ambrosial repast is the huigal, or fish flung into a pit, until it is *quite rotten*, when it is served up in a state of carrion, and with a stench, that is insupportable to every nose but that of a Kamschatkan.* These people, notwithstanding their mode of living, do, with the exception of the scurvy, enjoy good health.

"The Greenlanders and Esquimaux appear, by the accounts of those celebrated navigators, Davis, Frobisher, Baffin, Henry Ellis, &c., as well as Bishop, Egede, and Crantz, to live during the greater part of the year in very close, ill ventilated, and crowded habitations, (without chimneys,) which, notwithstanding the great severity of the cold, they keep extremely warm by their numbers and breath, assisted by a single burning lamp in each, and by excluding fresh air so completely, that any other people would think themselves in danger of being suffocated by the offensive vapours continually exhaling from the lungs and bodies of the inhabitants, and which involve them as a thick fog; and yet fever of any kind is a rare disease among these people, though, like those of Kamschatka, &c. they are much disposed to scurvy."†

Dr. Matthew Guthrie, physician at St. Petersburg, in a letter to Dr. Priestly, mentions, that "the Russian boor lives in a wooden house, caulked with moss, so as to be snug and *close*. It is furnished with an oven which answers the triple purpose of heating the house, dressing the victuals, and supporting on its flat top the greasy mattrass on which he and his wife lie."

"During the long severe winter season, the cold prevents them from airing this habitation, so that the air cannot be very pure, considering that four, five, or six people eat and sleep in one room, and undergo, during the night, a *most stewing process* from the heat and closeness of their situation, insomuch that they have the appearance of being dipped in water, and raise a steam and smell in the room, not offensive to themselves, but *scarcely supportable* to the person whom curiosity may lead thither."

* Bancroft, p. 100. † Ibid, p. 101.

"Now, if it be considered that this *human effluvium* must adhere to every thing in the room, especially to the sheep-skins or mattrass on which they sleep, the moss in the walls, &c., and that the apartment is never ventilated for six months, at least; at the same time that these people are living upon salt fish, and the whole time without fresh vegetables, &c.—if it be a fact that they are, in spite of all these predisposing causes, *strangers to putrid disease*, it will sufficiently justify my first assertion, that the regimen nature has dictated to these people, is highly antiseptic."

Bancroft remarks, that "Dr. Guthrie has stated these facts principally to show the supposed beneficial effects of the Russian drink called quass, &c.; but I am entitled to avail myself of them for the purpose of demonstrating that long confinement in close unventilated houses, without chimneys, in an atmosphere replete with human effluvia, and in very cold weather, when typhus or contagious fever is commonly most prevalent, does not produce that disease, it being, as will hereafter appear, unknown in that part of the world."* From these facts we learn that the effluvia arising from the crowding of healthy persons into close and unventilated situations, in cold countries at least, is not productive of fever; and upon examination, we shall find that the same cause is equally innocuous as regards the production of typhus fever in warmer regions of the globe.

The African slave ships present striking examples of this fact. "The poor wretches (says Lind, in his treatise on jail distemper) are crowded together below the deck, as close as they possibly can lie, with only a small separation between the men and women; every night they are shut up under close hatches, in a sultry climate, barred down with iron to prevent an insurrection; and though some have been suffocated by the close confinement, or foul air, though they are subject to the flux, and suffer from a change of climate, yet an infection is scarce known among them; or if an *accidental* fever, occurring from the change of climate, should become infectious, it is generally much more

* Bancroft, p. 102.

mild than in the opposite situation," i. e. that of ragged felons under transportation. Bancroft remarking upon this statement, says, "that Dr. Lind, influenced, as he was, by the commonly received opinions, mentions an infection (meaning fever) as being '*scarce known*' in the slave ships, instead of asserting, as he might have done with truth, that it is *never known*; for, (continues Bancroft,) after very extensive enquiries, I am fully convinced that fever of any kind rarely occurs on board these vessels, and *contagious fever never.*"

A striking instance of the harmlessness of the effluvia arising from the excessive crowding of persons into confined and close situations, may be found in the "Narrative of the deportation to Cayenne," of J. J. Job. Aime, and one hundred and ninety-two other persons, on board the Decade frigate, in consequence of the revolution (in France) of the 18th Fructidor, (Sept. 4,) 1797, written by himself, and printed in 1800. In this narrative the writer says, "we were placed in the between decks, before the fore mast and main mast, occupying nearly one fourth of the superficies of the vessel, having about four feet and a half in height, and receiving no light but by the scuttles; that is to say, by two openings of three feet square."

"In this place, the door of which was locked, were crowded and squeezed together one hundred and ninety-three individuals, mostly aged and infirm. We lay in two rows one over the other, forming, as it were, two stories, in hammocks of coarse cloth, and extremely narrow. Those below could not raise their heads without hitting those above; neither could any of us make the smallest motion without disturbing his neighbours; for we all touched each other, and not having the least spare room, formed, as it were, but one mass. And that nothing might be wanting to increase the horror of our situation, as we were not permitted to go out for fourteen hours together, (i. e. from 6 P. M., until 7½ A. M.) and sometimes more, tubs had been placed in the midst of us, where we might satisfy the indispensable wants of nature; and to get to these sorry receptacles, we were obliged to creep on our bellies beneath the hammocks. How insupportable then must have been the infection of such a close

confined place, which was already poisoned by our own exhalations! Indeed, the air which passed from this hole was so hot and fetid, that the sentinels placed at the hatchways as our guard, demanded that the time of their duty, at so dangerous a post, might be shortened."

In addition to all this, they "were condemned to subsist on the coarsest and the most disgusting half-putrefied food," and "to endure the greatest and most offensive personal filth."* Yet, notwithstanding this combination of the most disgusting and distressing circumstances imaginable, nothing like contagious fever manifested itself, although they were kept in this state for ninety-six days. If "crowding, want of ventilation, filthy clothing, and unwholesome, corrupting food, together with anxiety and dejection of mind," could have generated the contagion of typhus, it ought to have been present here in its most concentrated state.

Dr. Lind, in his essay on preserving the health of seamen, says, that "the surgeon of the Panther (of sixty-four guns) told me, that forty of her men had died of the scurvy on their passage home," (from the West Indies,) "and that during that time there were usually ninety patients in the sick apartment. The place appropriated for the sick was in the bay of the ship, and had no pipe from the ventilator, nor any scuttles cut through its sides for the admission of the fresh air. A number of patients, thus closely crowded together, rendered the place so disagreeable and suffocating, that the sick were in a manner stifled for want of air. The surgeon, when visiting, could scarcely breath it, or remain for any length of time, without being oblig have recourse often to the fresh air upon deck, and some to spirits of hartshorn, or to a glass of wine, for his imme relief. He observed, that both the virulence and mortal the scurvy were heightened by the uncultivated air of the in which the sick, for several weeks, had been confined out of above a hundred patients sent to the hospital by thi geon, not one was remarked to have any symptom of c

* Bancroft, p. 105–6.

generated in that apartment." This case is the stronger in point, because the men thus crowded together were in a diseased state. Many more facts of this kind might be mentioned, but these are deemed amply sufficient to satisfy the most sceptical, that human effluvium is not productive of contagious fever.

Of a Specific Contagion as the Cause of Typhus.—The contagiousness or noncontagiousness of typhus fever, has been the source of much, and occasionally bitter controversy, between the members of the medical profession. Until the present century, few or none were found so bold as to question its infectious character, and thus run counter to the prejudices, not only of the profession, but of society at large. Though, however, the doctrine of contagion, as an attribute of typhus, remained for a long time unimpugned; yet it finally came to be doubted, and the noncontagionists can now number as their friends a large proportion of the distinguished members of the profession. One reason of our long continued and implicit credence in the contagiousness of typhus, may be found in the very character of man. It is his nature to cling with great tenacity to long fostered and traditional opinions, clothed as they are with the names and authority of the venerable sages of other times; and he feels a pride in preserving his opinions, when once formed, unchanged; and thus it is that the prejudices of education, and the pride of consistency of opinion, often blind us to the light of truth, and rivet upon us the chains of error.

As it is a subject of much importance in many points of view, we will enter into the merits of this question as fully as the length of this essay will admit. The vexations, embarrassments, and restrictions upon commercial intercourse, produced by quarantine regulations, and the large and expensive establishments* in different countries for carrying them into operation, and the delay and inconvenience to individuals, all demand

*Maclean, in his work on Epidemics, vol. 1, p. 456, says, "In this country (i. e. England) they have cost the nation for the last fifteen years, independently of the detention of ships, crews, passengers, and merchandise, at least £300,000."

that this question should be rigidly examined into, and settled finally one way or the other. If, upon strict scrutiny, it should be found not to be contagious, then the abolition of quarantine establishments ought immediately to follow. If, however, after the most thorough and impartial examination, it should appear to be contagious, the quarantine regulations cannot be too rigidly enforced.

In discussing this subject, we shall in the first place contend that typhus fever has not the characteristics of a contagious disease; 2d, that it often has occurred without the presence of contagion; and 3d, that it often evinces no contagious character whatever. 1st. Typhus fever has not the characteristics of a contagious disease. All known contagious diseases run a regular and determinate course, and are marked by symptoms which unequivocally distinguish them from other diseases. Thus, smallpox, measles, chicken-pox, and cowpox have each peculiar symptoms, which succeed each other in regular succession, and pursue an invariable and certain course, independently of the treatment used. Thus, smallpox, for instance, though it may have its symptoms ameliorated by a proper treatment, or the reverse may take place by improper management, will, nevertheless, pass through all its regular and determinate stages. The same may be said of the other examples adduced. Now, the question may be asked, does typhus fever come up to this rule? We answer in the negative. That it has not distinctly marked pathognomonic symptoms, is evident from the fact, that different appellations are frequently given to the same fever by different physicians. Thus, one will pronounce it to be the common continued fever, another the genuine typhus, and a third, meeting with an unusually violent case, would call it yellow fever;* and there is not a single symptom laid down as belonging to typhus, which is not to be met with in other fevers.

* Dr. Carruthers, surgeon of the Malta, in his account of the typhus which prevailed on board that vessel, and which is to be found in Trotter's "Medicina Nautica," remarks, that "many of the sick had yellowness of the skin, insomuch that he suspected that the yellow fever had been introduced by communication with a sickly ship from the West Indies!" See Pathology and Therapeutics, By John E. Cooke, M. D. &c.

Neither does it, like known contagious diseases, run a defined and determinate course. It may last one, two, three, or six weeks, according to the treatment pursued. Again, the period at which morbid action is developed after the reception of contagion, is determinate in contagious diseases. This is not the case in typhus fever, as contagionists say that the infection may lie dormant during a period extending from one day to six months.*

In the next place, acknowledged contagious diseases affect a person but once during his life; this is a wise and beneficent law stamped upon them by the great author of creation to prevent them from becoming universally prevalent. "It is the incapability of affecting the same person more than once which, in general contagious diseases, sets boundaries to infection; and could alone, where no precautions are taken, prevent communities from being extinguished."† Typhus fever does not possess this characteristic; it may and frequently does, attack the same person repeatedly, and it is obvious that possessing, as it unquestionably does, this attribute, if you invest it also with the power of producing and propagating itself by a specified contagion, no human power could prevent its spreading almost over the whole human family. Every person in health would be attacked, as often as he came within the infectious distance of any one labouring under the disease. Those only would escape who could preserve themselves constantly beyond that distance, or were labouring under some malady higher, in degree, than the prevailing epidemic. But, as the disease spread, no

* Bancroft says the fever may make its appearance six months after the infection, and Haygarth expresses the same opinion. We cannot help applying a remark of Dr. Armstrong's to the opinions of B. and H. on this subject. He says, "If an author is persuaded that there must be a specific contagion as the cause of every fever resembling typhus, he will undoubtedly go far to seek it." The above named gentlemen prove the truth of this remark, for they, with the most wonderful acuteness and sagacity, pretend to have detected its operation six months after exposure to it. Unlike the bed of Procrustes, which reduces every thing to the same common size, they endow contagion with the wonderful property of expanding or contracting, so as to embrace every case which may occur from one day to six months after its application. Does this diversity in the time of producing its effects, comport with our ideas of something specific and unchangeable in its nature? Do the known contagions ever operate in this way?

† Maclean on Epidemics, vol. 1, p. 100.

person in health could continue long beyond the sphere of infection; and those who had been protected by diseases higher in degree, would, upon recovering, become liable to be affected by the contagion. It would observe a geometrical progression, diverging, as it were, from the centre to every point of the circumference of a city, a camp, an hospital, or a ship, while it had subjects to operate upon; i. e. while a single individual of the community remained alive it would never cease. Those who recovered would, again and again, be seized; no person who remained within the pale of society could escape. The malady would be communicated to the most distant nations. Infection would proceed in a continued circle, until the whole human race was extinguished."* This extract is a highly wrought, though true, account of what would be the effect of adding to the powers which typhus has of repeatedly attacking the same person, that of being capable of generating and propagating itself by contagion. Inasmuch, therefore, as typhus fever has no regular and pathognomonic symptoms, and as the period at which the symptoms of disease are manifested, after infection, varies, according to contagionists, from one day to six months, which is inconsistent with our ideas of something specific in its character, and uniform in its operations; and as it is unlike all known contagious diseases, in being capable of affecting the same person repeatedly, and does not spread until it becomes universal, which should be the inevitable effect of uniting to this latter trait the property of contagion, we think, therefore, it must be admitted that it does not possess the characteristics of contagious disease.

2d. That it has often occurred without the presence of contagion. To prove this second proposition, we will adduce facts mentioned by contagionists themselves. It is presumed that as the advocates for contagion would not gratuitously and without good reason mention facts subversive of their own opinions, that, therefore, these facts ought to have more weight than would be attached to them under other circumstances; as a

* Maclean on Epidemics, vol. 1. p. 214-15.

man is not to be suspected of insincerity or improper bias when he gives evidence against himself. But to the testimony.

Trotter, who was a zealous advocate for the contagious origin of typhus, mentions, in his Medicina Nautica, many instances of it, which could not be traced to contagion. On board the Niger, he says, "It first appeared among the marines, but latterly affected the seamen. Its origin could not be exactly traced."* In the Glory, also, "We could not trace it distinctly to its source."† "No imported contagion was duly authenticated"‡ as the cause of the fever which broke out on board the Edgar, while at sea. Of that on board the Pompey, at sea, "No satisfactory account could be obtained of its origin."‖ Trotter says, Mr. Johnson remarks in relation to the fever which occurred on board the Saturn, "I could not trace this fever to any infection imported by any of the people."§ In the Magnificent, the surgeon says, the fever spread more than he had expected; "but still I cannot trace it satisfactorily to a contagious cause."¶ Trotter says, when speaking of the typhus on board the Captain, "No imported contagion has been suspected here."** Of the same on board the Uranie, he says, "It does not appear that any imported contagion has been suspected as the cause of this fever."†† Speaking of the same vessel again, he says, "It does not appear that any imported contagion could be suspected here." In giving the result of his experience for many years, he says, "It is very certain that this fever is generally spread by contagion; but it is equally certain, that it frequently *arises in places where there could be no suspicion of communication with infected persons or clothing.*"‡‡ The above facts are taken from a treatise on pathology and therapeutics, by John E. Cooke, M. D., professor, of Transylvania University, in which this subject is discussed with the most masterly ability. I shall mention but one more case of this kind, to be found in Assalini's work on the Plague. He says,

* Medicina Nautica, vol. 1, p. 153. † Ibid, p. 160. ‡ Ibid, vol. 3, p. 63.
‖ Ibid, p. 64. § Ibid, p. 147. ¶ Ibid, p. 167.
** Ibid, p. 60. †† Ibid, p. 151. ‡‡ Ibid, vol. 1., 252.

that in the years 1799 and 1800, "An epidemic disease spread its ravages in the hospitals of the army of Italy, and carried off daily its numerous victims." The fever spreading rapidly, and much alarm being created, "the public authority (says he) thought proper to consult the school of medicine of Montpelier, which hastened to calm their inquietude, and reanimate their drooping spirits, by proving that this fever, falsely regarded as pestilential, was not at all different from the fever of hospitals, the *typhus carcerum* of Pringle, or the fevers of camps and armies, febris castrensis." "With regard to this disease, the majority attributed it to rains and fogs. A citizen of the environs of Antibes assured me, that if this disease had taken place some months before, it would, without fail, have been attributed to the vessels just arrived from Egypt, and which had put ashore at Frejus without performing quarantine."* These facts are deemed sufficient to prove that typhus often appears without the least reason to suspect contagion as its cause.

Physicians, however, aware of this fact, and unwilling to surrender their long cherished opinions, are driven to the absurd alternative of supposing that typhus may have a double origin, or, in other words, that the specific contagion of typhus may be produced by other causes than itself. Thus, Gregory says, "there is nothing improbable in the supposition, that what originated in cold may be afterwards propagated by contagion."† Dr. Adams, in his work on morbid poisons, inculcates the same opinion. To these, and all similar opinions, we will apply the language of Bancroft when alluding to that of Dr. Adams on this subject. He says, "that Dr. Adams, who is accustomed and qualified to reason, should have believed any thing so unphilosophical and *incongruous*, would have been incomprehensible to me, if so many others had not discarded common sense on the subject of contagion. To represent a disease which is *notoriously contagious*, and propagated by contagion, as capable of being produced by other, and those very different, means, is

* Assalini on the Plague, p. 89, 90, 91.
† Gregory's Practice of Physic, vol. 1, p. 55.

to multiply causes unnecessarily, and therefore unjustifiably; and it moreover destroys the natural and just influence of causes upon their effects, by making the same disease result from very dissimilar causes; and, a little farther on, he observes, that "were it possible for typhus thus frequently and easily to originate without contagion, and at the same time acquire and multiply itself by a contagious quality, who could ever hope to escape the disease."*

3d. That it often evinces no contagious character whatever. Contagionists themselves afford evidence to this effect. Trotter, in his Medicina Nautica, mentions many instances in which it was not contagious. "A fever (says he) of the typhus kind appeared among the gentlemen of the Invincible, in Torbay, which extended to a number of cases. Yet it is singular, that it did not affect any of the seamen, although some of them attended their officers, who slept in the gun room, and where communication with the ship's company was not prevented."† "The Niger has been ten weeks at sea; at leaving port four relapses in fever happened among men discharged from the Warlar. Mr. Burd does not assign any cause for these returns; no infection spread from them."‡

"We know of nothing (says Trotter) that can propagate this fever, but exhalations from the body. But a patient in typhus was sent from the Venerable to the hospital ship, with a fetor about him, that exceeded every thing of the kind which ever came within my knowledge. After being washed and shifted, it still continued, and was perceived at a considerable distance. He died in a few days; yet nobody was infected from him, either in his own ship, or our hospital."‖

Speaking of the Charon hospital ship, Trotter says, "We had in that hospital many malignant cases of typhus, and some deaths, yet no infection was ever spread there."§

In 1790, a number of troops embarked on board the Gorgon

* Bancroft on Fevers, p. 340, 341.
† Trotter's Medicina Nautica, vol. 1, p. 109.
‡ Ibid, p. 164.
‖ Ibid, p. 195.
§ Ibid, p. 179.

to go to New South Wales. While lying in the harbour of Portsmouth, typhus fever prevailed among the soldiers, and several died. "The troops lived on the lower deck, and when the ports were shut at night, the exhalations from below naturally ascended through the gratings among the sailors; not one of them had the slightest sign of infection."* The above facts are also derived from Cooke's work on pathology and therapeutics.

In the instance already cited from Assalini, of the fever which attacked the army of Italy, in the Ligurian republic, it is stated in a report made to the society of the school of medicine at Paris, that "it was of the nature of jail fever, and was not at all contagious."†

Wilson, in a note to page 156 of his chapter on continued fevers, says, "It is remarkable, indeed, that we sometimes meet with malignant fevers, which do not appear to be at all contagious." "Sometimes, (says he,) Dr. Lind observes, one man may be seized with the petechial or with yellow fever, while the rest continue unaffected." Dr. Lind gives several instances in support of this observation.

These facts prove that typhus fever is not, at least, always contagious, and it is inconsistent with our ideas of a strictly contagious disease, that it should at one time possess this characteristic, and at another be destitute of it. Known contagious diseases are always and invariably the same; they are contagious under all circumstances; but typhus fever has been shown not to be so; and, to use the language of Bancroft, "as the same disease, in suitable circumstances, is *always*, or *never*, contagious, we may safely rely" on the numerous facts adduced, to disprove the assertion, that contagion is one of the characteristics of typhus.

The advocates, however, for the contagious nature of typhus, in order to avoid the inference legitimately deducible from the above facts, and others which exist, are driven to the necessity of reducing the question of its contagious character to very narrow limits. They admit that it does not evince this

* Trotter's Medicina Nautica, p. 210. † Assalini on the Plague.

quality in situations where there is a free circulation of pure atmosphere;* but contend that it possesses it in the impure atmosphere of close, filthy, and unventilated apartments. The fact of its attacking many persons in succession under such circumstances, has been assumed as sufficient ground for a belief in its contagious character; but this surely only proves the existence of some general cause acting on all in those circumstances, and not that the disease is communicated from one to the other by means of a specific contagion. That general cause is unquestionably the impure atmosphere itself; for, if an individual labouring under typhus is removed out of the foul air, he does not propagate the disease; but the impure atmosphere will continue to produce it in most of those subjected to its influence. The circumstances, then, under which alone contagionists say that typhus is infectious, being of themselves capable of producing it, or exciting it, are in them already predisposed; but it is unnecessary and unphilosophical to assign that as a cause of the fever, which is acknowledged to be only operative during the presence of causes which, without this, are capable of producing it. We do not think, therefore, that contagionists can sustain themselves on this ground. Under the next head of this essay we will speak more particularly of the cause of this impure atmosphere, which we consider as alone capable of producing this fever.

We have now finished what we have to say on this part of the subject. We trust that we have established to the satisfaction of every unprejudiced mind, the three positions for which we have contended. 1st, that typhus fever has not the characteristics of a contagious disease; 2d, that it has often occurred without the presence of contagion; and 3d, that it often evinces no contagious character whatever. If we have even proved the last of our positions, then Bancroft and his disciples at least ought to admit the justice of our conclusion that it is never

* It was from a knowledge and conviction of this fact, that infirmaries and houses of recovery were established in England. These institutions for the reception of the wretched paupers of that country, were the result of the philanthropic labours of Ferriar, Percival, and others; and will constitute a lasting monument to their memory and reputation.

contagious, as he uses the same train of arguments against the contagious nature of yellow fever, and comes to the same conclusion in regard to it, from the facts he had collected upon that subject.

There are, it is true, some apparently strong facts on record in proof of its infectious nature, but we believe they cannot stand the test of rigid scrutiny. The fevers which occurred at the black assize, at Oxford, in 1577, and at the Old Bailey, in 1750, are always held up as evidences of its contagious character; but no one can read the remarks of Bancroft upon them, without being convinced of their utter want of claim to be considered in this light. To notice all the instances of this kind to be found in books, would swell this essay far beyond reasonable limits, and we shall therefore not attempt it.

We will now, in the last place, examine the opinion of those who deny that typhus fever possesses the property of contagion, and who consider it as a mere variety of the continued and remittent fever, and of course as arising from the same causes.

We are among those who entertain this opinion; and from a close and impartial examination of all the facts and arguments bearing on the question, are perfectly satisfied of its correctness. In order to render this subject as clear as possible, we will in the first place endeavour to show "that typhus fever arises in situations in which miasmata abound." 2d. "That the symptoms of typhus are those of autumnal or miasmatic fevers." 3d. "That typhus fever appears in company with miasmatic diseases, and that they are mutually convertible into one another."* Before proceeding, however, to a consideration of the different heads of this part of the subject, it will be just and proper to remark, that we shall stand indebted to Dr. Cooke's admirable essay on the causes of typhus, contained in the first volume of his work on pathology and therapeutics, for most of the facts to be mentioned on this subject. The author of the above mentioned work has so completely covered the whole ground of controversy, and so perfectly exhausted the subject,

* Cooke's Pathology and Therapeutics, vol. 1, p. 450.

that but little else is left for others to do, than merely to draw upon the rich storehouse of facts, collected by his unceasing and indefatigable industry.

1st. Typhus fever arises in situations where miasmata abound. Currie mentions, "that the corporation of Liverpool, being about to apply to parliament for powers to improve the streets and police of the town, requested the physicians of the Infirmary and Dispensary to suggest to them such alterations as might contribute to the health and comfort of the inhabitants.* The physicians took this request into serious consideration, and presented a report of considerable extent, including a view of *the causes* of the uncommon sickness of the two preceding years, and of *the measures requisite to prevent its recurrence*, and to remove the frequency of contagion in the habitations of the poor." The physicians consulted, and pointed out the necessity of enforcing cleanliness in the streets, to which end an improvement of the pavement was represented to be essential; and they particularly advised a general review of the common sewers, and an improvement of their structure, on the principles of the report on this particular subject, addressed by them to the mayor and magistrates, in 1788. They further advised, that effectual provision should be made for draining the grounds within the liberties, and particularly to the north of the town. "Repeated remonstrances (the words of the report) have been made for the last twenty years, on the collections of standing water, including filth of every kind, which are suffered to remain in the district which extends along the termination of the streets, from St. Paul's square to Byron street, and to which the low fevers, which in the autumnal months especially infect these streets, are principally to be imputed."† In this instance, the physicians, with a view to prevent the recurrence of the "low fevers" which had prevailed so widely during the two preceding years, recommend the removal of those circumstances, which are every where fruitful in the production of miasmatic exhalations.

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* Currie's Med. Reports, p. 246. †Ibid, p. 247.

At page 23, Currie mentions another fact, clearly evincing the miasmatic origin of typhus fever. He says—"The 30th regiment, as is usual with troops in Liverpool, was billeted in the town, but paraded and mounted guard in the fort, situated north of the town, on the banks of the river. The general guard room had been used, previous to the arrival of the 30th, as a place of confinement for deserters: it was extremely close and dirty, and under it was a cellar, which in the winter had been full of water. This water was now half evaporated, and from the surface issued offensive exhalations." In a dark, narrow, and unventilated cell of the guard room, it was usual to confine such men as were sent to the guard for misbehaviour; and about the 20th of May, 1792, several men had been shut up in this place on account of drunkenness, and suffered to remain there twenty-four hours, under the debility that succeeds intoxication. The typhus or jail fever made its appearance in two of these men about the first of June, and spread with great rapidity.* All the precautions used, by cleansing and ventilating the guard room, were ineffectual in preventing the progress of the fever. The weather was at this time wet, and extremely cold for the season: the men on guard could not be prevailed on to remain in the open air; and, from passing the night in the infected guard room, several of the privates of the successive reliefs caught the infection, and fell ill on the 10th, 11th, and 12th of the month.* We here find that all attempts at arresting the fever by washing and ventilation, were in vain; and why? because the source of the fever obviously was the "offensive exhalations" from the cellar under the guard room; and it was not until the guard room was shut up, and the soldiers removed to "a temporary shed erected in its stead," that the fever ceased.

Dr. Ferriar, of Manchester, in England, in his address to the police on the best means for the prevention of fevers, remarks, "that during the late epidemic, it was observed that the fever (i. e. typhus) prevailed most in streets which were not drained, or in which dunghills were suffered to accumulate, or where

* Currie's Med. Reports, p. 25.

the blood and garbage from slaughter houses were allowed to stagnate." These facts, and many others of a similar import which we might mention, unquestionably prove that typhus fever arises on land, in situations where miasmata must abound.

It has also frequently appeared in ships at sea; and after a full and rigid examination of many instances of the kind to be met with in books, we think we can venture to assert that in every instance, where the circumstances attending its origin and progress in such situations are detailed, they will be found abundantly sufficient to produce fever, without bringing in contagion to account for it. One of two things will be generally found to exist; either that the state of the vessel as regards filth, &c. is such as would probably of itself give rise to febrific exhalation; or, that the crew has been exposed to miasmata on land, and being thus predisposed to fever, the exposure to bad weather, the inclemencies of a sea voyage, and the fatigue incident to such situations, act as exciting causes, and fever is the result.

2d. That the symptoms of typhus are those of autumnal or miasmatic fevers. Under this head we shall say but little, because we believe that no one will question its truth, who examines into it with any degree of attention. Dr. Armstrong, in speaking of the identity of the remittent and typhus fevers, remarks, that the remittent and typhus fever from *malaria* or *marsh effluvium*, "has a combination of symptoms exactly similar to those which occur in continued typhus fever, and which, *as a combination*, occur in no two other affections whatever."* They both commence with a stage of oppression or weakened action of the heart; in both this first stage occasionally continues unchanged, and constitutes then the congestive fever; but in both the heart generally reacts from this first stage of oppression, and produces the open form of fever, the succeeding symptoms in each being dependent on the degree of this reaction, and the previous state of the different organs of the body.

The general symptoms in both are, therefore, alike, and if

* Medico-Chirurgical Review, September No., 1822, p. 295.

we descend to an examination of particular symptoms, we shall find none which exclusively belong to typhus. Petechiæ, which are so common in typhus, as to have given it a name, occur also in other fevers. Rush mentions their frequent appearance in the yellow fever of 1793, and almost every practitioner of any experience has often met with them in the autumnal fever of this country.

Yellowness of the skin is a frequent symptom of miasmatic fevers. It also often occurs in typhus fever. Mr. Caruthers, surgeon of the Malta, in his account of the typhus which had appeared in that vessel, says, that "many of the sick had yellowness of the skin, insomuch that he suspected that the yellow fever had been introduced by communication with a sickly ship from the West Indies."* In reference to the above statement, Trotter remarks that "Icterus is not an unfrequent symptom in typhus; it was particularly remarked among our seamen in the warm summer of 1794;"† and in vol. 1, p. 259, he says, "I have often seen the eyes and skin, with the urine, as deeply tinged with bile in typhus, as I have remarked them in the yellow fever of the West Indies."

We will now proceed to the consideration of the third and last division of this part of our subject, which is "That typhus fever appears in company with miasmatic diseases, and that they are mutually convertible into each other." To prove this we shall resort to the testimony of those who were perfectly familiar with the disease in all its different forms.

It is now generally acknowledged that dysentery is but a form of our autumnal bilious fever, and, of course, that it originates from the same cause, or, in other words, that it is, what the unerring sagacity of Sydenham long since pronounced it, a "febris introversa." Typhus fever, then, often appears in company with this disease. Pringle, in his account of the diseases of the army when in Germany, in the year 1743, says, "The state of those at Feckenheim has been already mentioned: there the hospital fever and dysentery grew daily worse."‡

* Trotter's Medicina Nautica, vol. 3, p. 215. † Ibid, p. 218.
‡ Pringle on Army diseases, p. 25.

Trotter says, the ship Reasonable arrived at Spithead in January. 1794, and "landed upwards of a hundred very ill of typhus and dysentery." "A considerable number of cases of flux and (typhus) fever, were received from the Gibraltar."*

At page 192, of vol. 1, of his Medicina Nautica, Trotter says, "There were a larger number of bad cases of typhus, ague, and dysentery, than usually come to a naval hospital at one time. From the same transport, and in the same regiment, were brought people ill of the three diseases just mentioned;" and in vol. 2, p. 14, he says, on board the Powerful, "a dysentery was associated with the fever."

It also appears in company with remittent and intermittent fevers, which are confessed by all to be produced by miasmatic exhalation, and is often converted into them, and *vice versa.*

There is an abundance of evidence in proof of this. Trotter says, "I have seen a family where the father was labouring under an obstinate tertian, while the mother and some of the children were ill in bed with typhus." "Nay, I have constantly remarked in those ships where contagion prevailed, that many cases of regular intermittents and remittents occasionally appeared. When they increased in proportion to the number of the continued type, and the latter becoming milder in its attack, I consider it as an infallible sign that the power of the infection was on the decline, and would be speedily subdued."†

"The attacks of fever," says Trotter, "since leaving Cork, had been more numerous than on the preceding week. They were both of the continued and intermitting types."‡

About the beginning of March, the infection seemed to be on the decline; which appeared from the attacks being less severe; some cases of the remittent and intermittent type were now observed.‖

"The sick list consisted of seventeen venereal patients, and seven in fever, some of them regular tertians. Some were every morning discharged to duty, and others added in the

* Trotter's Med. Nautica, vol. 1, p. 58. † Ibid, p. 185.
‡ Ibid, p. 311. ‖ Ibid, p. 187.

course of the day, in both the continued and intermittent forms."*

"Those cases (of typhus) which have occurred latterly, have been slight, with a general intermittent tendency."†

"The Royal George did not seem to suffer much from the infection." "They assumed a bilious remittent form."‡

Trotter says, "It is not easy to trace those circumstances, which occasionally convert an intermittent or remittent fever into a continued type, (or typhus,) and *vice versa*. There must, however, be something in their disposition very much alike; for, wherever we find the typhus affecting a number of people at a time, we also find cases of the remittent and intermittent form."‖

Ferriar mentions one instance of the conversion of typhus into intermittent fever. "In one case," says he, "where a typhus was unusually protracted, after several hazardous determinations to the stomach and bowels, the fever assumed the form of an intermittent."§

Trotter, in speaking of the typhus on board the Valiant, says, "the cases were becoming milder; in some it put on the intermittent form, which showed that it was now on the decline."¶

Dr. Armstrong, in a short essay on the origin, nature, and prevention of typhus fever, published in 1822, made a public recantation of the opinion he had long entertained relative to the contagion of typhus, and adopted that of its miasmatic origin, and its identity with intermittent and remittent fevers. The following account of the reasons of his change of opinion, is taken from the Medico-Chirurgical Review, September number, 1822. "Dr. Armstrong states, (says the Review,) that in 1819 he attended a patient labouring under an intermittent fever, which in its progress, put on a remittent character, and ultimately a continued form and typhoid type, accompanied by very malignant symptoms. This case made a deep impression on his mind, and inclined him to ask, whether intermittent, re-

* Trotter's Med. Naut. vol. 1, p. 188. † Ibid, p. 70. ‡ Ibid, p. 83.
‖ Ibid, p. 311. § Ferriar's Med. Histories, p. 82.
¶ Trotter's Med. Naut., vol. 1, p. 66.

mittent, and typhus fevers, might not possibly be modifications of one and the same disease. Up to this period Dr. A. had firmly believed that human contagion was the sole cause of genuine typhus fever; but a doubt having been excited, he determined, if possible, to divest his mind of all former bias, and investigate the subject ab origine. In the course of the three succeeding years, a very great number of typhus cases have fallen under his notice, and the result of his observations is a decided conviction," "that what the Italians vaguely call *malaria*, and the English, as vaguely, *marsh effluvium*, is the primary source of typhus fever." In the next page the Review continues the subject, and says, "In tracing back many of the cases, Dr. A. found that they had commenced as intermittents; he has seen the remittent run into the continued typhus, and the continued typhus become remittent or intermittent."*

What stronger testimony can any one require to prove the identity of any two diseases, than the fact of their frequent conversion into each other? We have cited abundance of evidence to prove that this frequently takes place in typhus and acknowledged miasmatic diseases, and it is a fact, too, which cannot be evaded, unless by those who are absurd enough to contend, that a disease which they say is one *sui generis* and specific in its character, can change to one entirely different in its nature and origin. If there be any such persons, we will only ask them, if smallpox was ever known to change into measles, or *vice versa*, or cowpox into chickenpox?

It has been the common opinion for a long time, that typhus fever is a disease exclusively of the winter season, and hence the conclusion that it cannot arise from marsh effluvium, because the latter requires for its production, the cooperation of heat with moisture. We however, deny the fact of its being a

* This repudiation of a long cherished and favorite opinion is highly honorable to Dr. Armstrong; it shows a mind truly independent and devoted to truth, and clearly proves that he will not cling to an error, when convinced of its being such, merely for sake of consistency of opinion. Oh! si sic omnes. It, moreover, speaks volumes against the contagious nature of typhus. Dr. Armstrong never would have left the ranks where he had long fought as the most distinguished champion, without the most strong and irresistible reasons for his conversion.

disease only of the winter season, and a reference to the different works on the subject will support us in the denial. Currie and Trotter mention many instances of its appearance and most extensive prevalence during the hot weather of summer and autumn, and Armstrong says he has known it to prevail during the greatest heats of summer. 'Tis true, that it prevails almost every winter, more or less, among the poor population of almost all the large cities both in England and this country; but, upon enquiry, it will be found that those attacked have most generally been exposed to the sources of miasmatic exhalation during the preceding summer or autumn, and having thus acquired a predisposition to disease, the complicated and depressing miseries of poverty and want act as powerful exciting causes, and fever is produced.

We have now finished the subject of these pages; we cannot, however, say to our own satisfaction. We are concious that much has been left unsaid, the ordinary limits of an essay not admitting of any thing like a wide range of discussion. We have endeavoured to cull from the various works on the subject, the most striking and prominent facts bearing on the different subjects embraced in this essay. A considerable portion of it has been devoted to an examination of the different assumed causes of typhus, because we consider the possession of correct notions on this subject a matter of very great importance. Whether the conclusions we have arrived at are correct or not, must be left to the decision of others.

ART. II.—*Thoughts on Generation*, By CHARLES CALDWELL M. D.

This memoir will bear no very remote relation to chapter XII of Dr. Bostock's "System of Physiology," in which he treats of the subject of Generation. Although it will not be a real review of that chapter, it may be considered somewhat in such a light, in as much as it will derive from it one or two of its topics of discussion, and contain a few strictures on certain views which its presents.

Generation pertains to living organized matter alone; because that alone dies and requires to be replaced in constant succession. It belongs exclusively, therefore, to the vegetable and animal kingdoms. Minerals do not generate, because they do not die. If, to these remarks, there exist any exception, real or seeming, it presents itself in what is denominated *spontaneous* generation, to which I shall briefly advert, in the close of this article.

Generation may be defined that process, by which, out of some portion of one living being, is formed another of the same species. As relates to the higher orders of animals, different parts or stages of this process are designated by different terms. These are, in particular, impregnation and conception, gestation and parturition. The two former constitute the commencement or first act of the process, one on the part of the male, the other of the female; and of them it is my purpose more especially to treat. The others, if spoken of at all, will be introduced only incidentally, and by way of illustration. It is with the same view, that, although my remarks will be intended exclusively to throw light on the economy of the higher orders of living matter, I shall refer occasionally to generation in some of the lowest. For although analogy must not be relied on, as positive proof, it strengthens probability, and is the source of illustration. He who has not attempted to draw in to his aid, all collateral and tributary lights, and to enrich himself with facts from every department of living matter, has no just title to the name of a physiologist. His views, like the field he has

traversed, are narrow and partial. They are limited to individualities and points. To know mankind thoroughly, it is requisite to travel and mingle with the people of different countries. In like manner, to become extensively versed in physiology, it is essential to pass beyond the sphere of the more perfect animals, and study the economy of the less perfect. So fallacious is the notion, although it has many advocates, that the functions of the lower have no fitness to shed light on those of the higher kinds of organized beings. As well might it be contended that a knowledge of the alphabet is not necessary to the art of reading; or that, in any case, an acquaintance with the elements of a branch of science, as simples, is not essential to a knowledge of that science, in its more complex character. It is only in the lower orders of living matter that the elements or component parts of physiology are found in a separate and simple condition. It is there that we must look for the alphabet of the science.

I need scarcely remark, that, in the class of animals of which it is my purpose to treat, generation is connected with a difference of sex. Nor shall I specify the points in which that difference consists. With all these details the reader is presumed to be already acquainted, in a degree sufficient to render my observations and reasonings intelligible to him. Some of the sexual differences, however, must be occasionally brought into view.

As relates to certain terms, by which generation is usually characterized, I deem it not altogether unimportant to express my opinion, because I consider them productive of mischief. Scarcely a writer treats of generation, who does not preface his disquisition, by a few very grave remarks about its "mysteries," a word which is paralysing to rational inquiry. If the process be, as it is pronounced, positively *mysterious*, the less time we devote to the investigation of it the better; for it is time thrown away. Mysteries are transcendental; above the reach of human powers; have no affinity to observation or reason; and should be, therefore, let alone, except so far as they may be acted on by *faith*, a term which has no place in the

nomenclature of physiology. A *scientific faith* belongs only to the indolent, the feeble, or the irresolute. Bold and enlightened inquirers reject it. Hence those writers and teachers, who are most profuse, in words, about the "mysteries" and the "*arcana*" of nature, rarely do much, in fact, to *unmysterise* the one (if I may be permitted to compound a word for the occasion) or to draw the other from concealment to light. The very adoption and free use of such terms would seem intended as an apology for careless effort, and superficial research. To represent a function or an object as inscrutable, is to weaken if not destroy, in the reader as well as the writer, and in the hearer as well as the speaker, all liberal ambition and efficient endeavours to scrutinize it.

Nor is this all. It is not, perhaps, even the worst. This mysterising practice betrays the unthinking into a fundamental error, which leads to many others. It induces them to believe that generation is a process so completely insulated in its nature, as to be performed on principles totally different from those of every other function of living matter. It persuades them that it is, in all respects, so perfectly *sui generis*, that, in attempts to elucidate it, no aid is derived from the most intimate knowledge of other processes, whether animal or vegetable. This, I repeat, is not only itself a mistake, but is a prolific source of many others. It renders most of what have been denominated the "Theories of generation," an entire tissue of mistakes; some of them as gross and extraordinary as imagination can conceive.

It is not true that generation is a mystery; or if it be, so are the animal and vegetable economics in all their details. So is respiration, digestion, nutrition, growth, secretion, sensation, muscular motion, and every other function of organized matter. We know as much of generation as we do of them. It is not more extraordinary that a portion of one living being should, by a specific kind of action, be converted into another, than that a piece of dead matter should, by peculiar and appropriate action, be converted into living. It is not more wonderful that, by the process of generation, a beautiful woman should become

the mother of a beautiful child, than that by means of digestion, assimilation and nutrition, an unsightly mass of pork, beef, cabbage and potatoes should be converted into that same beautiful woman. It is not less extraordinary that, after the birth of her child, she should still continue to contribute to the support and growth of all its organs, however discrepant from each other in character and form, by the simple secretion of one set of glands, than that she should, by another set, have secreted originally its fœtal rudiments. Nor is there any thing more "*mysterious*" in the results of the generative process, than in the maintenance of flowers and fragrance, by earth, water, and air, or the conversion, by the digestive and nutritive processes, of marble into the terebellum marmoreum, or of common stone into the terebellum saxosum. Digestion, then, I repeat, is as much a mystery, as generation; and I will add, respiration is as much so, as the process of thought. We know as well how the blood of a female can be changed into the rudiments of a fœtus, as how it can be changed from venous to arterial, or the reverse.

As relates to the *quomodo*, or specific mode of action, by which it is effected, every function of living matter is a mystery to us; but, as respects its principles, the laws by which it is governed, and the influences which modify it, no function is mysterious. When viewed in the latter point of light, all of them become intelligible and plain. This is as true of the generative process, as of any other pertaining to the economy of living matter. And it will appear, hereafter, that that process depends on the same principles with every other which such matter performs. It is exceedingly desirable, then, that all terms and modes of expression, tending to mysterise, *in appearance*, and cover with error, *in reality*, should be abandoned by those who are friendly to science, who delight in liberal and manly discussion, and whose object is truth. There is no other word, in our language, which presents such an obstacle to intrepidity of thought, freedom of research, and the progress of knowledge, as the term "mystery." On common minds it sits like an incubus, or acts like the meteor that leads from their path the unwary and the credulous. For the bene-

fit of society, and the honor of these reputedly enlightened times, the evil should be remedied.

As matter further preliminary to the discussion on which I am about to enter, it is necessary to state a few general propositions, so much in the character of axioms in physiology, that simply to announce them is all that will be requisite. To the enlightened physiologist they will exhibit, at first sight, conclusive evidence of their own truth, and can be applied by him, as principles, to the elucidation and establishment of other truths.

1. All living action, whether in animals or vegetables, is the result of *stimulation*. Without this, *life in the abstract*, I mean matter possessed of life, can exist; but it must necessarily be in a state of inaction. It resembles caloric in a latent condition. That it may exhibit functions, of any description, stimulus must be applied.

2. That they may perform their functions healthfully and with vigor, many organs require specific stimulants, without which they will either not act at all, or they will act morbidly and feebly. To every organ in the body two stimulants are essential. These are caloric, or a certain degree of temperature, and the fluid conveying to them life and nourishment, which is the blood in animals, and the sap-juice in vegetables. To maintain the functions of many organs, the agency of these two stimulants alone appears to be sufficient. They might be denominated stimulants in ordinary.

3. But although all organs require, for the performance of their functions, the agency of stimulants, they do not require that those stimulants be applied immediately to themselves.

4. By virtue of a law of living matter denominated *sympathy*, the existence of which every medical philosopher acknowledges, and the influence of which every individual constantly feels, remote impressions, made by stimulants, excite as certainly and powerfully, as those that are proximate. In more explicit terms, impressions made on a part remote from a given organ excite that organ to action as effectually as if they were made on the organ itself. This is a most important law, and must

never be forgotten by those who would understand aright the operations and phenomena of living matter. Facts in illustration and proof of it will be adduced hereafter. For my present purpose to announce it is sufficient.

5. In no part of the animal body does this law prevail in greater force and perfection, than in the generative system of the female, consisting particularly of the vagina, uterus, Fallopian tubes, and ovarium. By exciting, in the proper mode and degree, one of these organs, appropriate action is always produced in some of the others. Were proof of this wanting, it might be found abundantly in the experiments of Dr. Haighton, published in the Transactions of the Royal Society of London, for the year 1797. Other testimony to the same effect might also be adduced.

6. The ovarium, which is a glandular body, is the immediate seat of impregnation and conception. It contains a number of small vesicles, each of which is a gland, intended for the formation of the rudiments of a fœtus. Proof of this also is afforded, by the experiments to which I have just referred.

7. To produce the excitement requisite to such formation, the semen masculinum is the proper stimulus. Without the suitable application of this, the fœtal rudiments cannot be formed; precisely as vision cannot be produced without the stimulus of light, the action of the heart without the stimulus of the blood, nor taste without that of sapid bodies.

8. The production of the rudiments of a fœtus is as much the result of a secretory process, as the formation of bile, pancreatic juice, or saliva. It is impossible that it can be the result of any thing else. Those rudiments, whether they are the product of the paternal or the maternal system, come from the blood. And it is secretory action alone that can form them. What organ or organs are chiefly concerned, in the performance of that action, is a question of great interest, which remains to be considered.

The fitness of the preceding propositions to shed light on the subject under discussion will appear hereafter. The point, in this subject, which gives rise, at present, to the greatest discre-

pancy of opinion among physiologists, is the action or province of the semen masculinum; the part which it really performs, in the process of generation. This question demands and shall receive my earliest attention. It constitutes the gordian knot of the inquiry. The solution of it is a sure earnest of success with respect to most other difficulties that may present themselves. As relates to the office and operation of the semen masculinum, four distinct opinions have been entertained.

1. The animalculæ, with which that fluid is known to abound, have been considered real *homunculi*, formal germs of men and women, perfect in all their parts, and requiring only a nidus and nourishment, for their complete evolution. The nidus where nourishment is prepared and administered is the uterus. According to this hypothesis, the male has performed his generative duties, when he has introduced into the uterus one or more of these homunculi.

2. According to another class of physiologists, the fœtal germ belongs to the female. It is primitively lodged in the ovarium, but does not possess the requisite kind or amount of life, to proceed in growth, and attain maturity, without the agency of a specific stimulus. Such a stimulus is the semen masculinum. The peculiar and only province, then, of that liquid is, to awaken to life and action the hitherto dormant fœtal germ.

3. A third hypothesis is, that toward the formation of the fœtal rudiments, the female also furnishes a fluid, which is deposited in the uterus. With this the liquid of the male is mingled, when, by the joint and reciprocal action of the two, the rudiments are produced.

4. The advocates of a fourth theory deny the pre-existence of fœtal germs, both in the male and female. Such germs, they contend, are the product of impregnation and conception alone. Impregnation is the act of the male, and conception of the female. The ovarium is the seat of both; and both are produced by the specific stimulation of the semen masculinum. This is the theory to which I am myself an adherent, and which I shall endeavour more fully to develop and elucidate.

To the four preceding might be added a fifth hypothesis,

which, although exceedingly fanciful, found a strenuous advocate in the celebrated Harvey, if it does not claim him as its author. It is, that the semen masculinum emits, from the place where it is deposited, *in coitu sexuum*, a subtle *aura*, which, by a contagious quality of which it is possessed, reaches the ovarium, and impregnates the female. So visionary is this hypothesis, that, notwithstanding the high authority of its defender, its annunciation and reputation would seem to be one. Of the hypothesis of Buffon the same is true.

It is not my purpose, in a minute examination of the three first of the foregoing theories, to point out their errors. The attempt, besides tasking me with a large amount of stale and unproductive labour, would compel me to extend this memoir to an unwarrantable length. The fourth theory, which, as already mentioned, I believe to be alone founded in truth, is the only one which I shall freely discuss. But as the third finds an advocate in Dr. Bostock, a writer of some note, who has declared in its favour, in his late elaborate "System of Physiology," it is becoming in me, at least, if not necessary, to make it the subject of a few observations.

This however I shall not do, in a spirit of direct reply to its learned author. Such a step would be almost impossible. For although the Doctor is profuse in his details of the opinions of others, he is exceedingly parsimonious with regard to his own. So delicate is he, in fact, in expressing his opinion, as almost to excite a doubt, whether he possesses any; at least, whether he has any in which he is decided. Indeed to such an extent does he carry this hesitancy, in telling the reader what he positively thinks, and so incongruous are some of his statements, as to authorize a suspicion that he thinks differently, on the same topic, at different times. A specimen of the Doctor's want of decision we have in the following paragraph, where he is discoursing about the probable seat of impregnation.

"Perhaps the most natural supposition may be, that the ovum is transmitted to the *uterus* in an unimpregnated state, but there are certain facts which seem almost incompatible with this idea, especially the cases which not unfrequently occur of perfect

fœtuses having been found in the *tubes*, or where they have escaped from them into the cavity of the abdomen. Hence it is demonstrated that the ovum is occasionally impregnated *in the tubes*; and we can hardly resist the conclusion that it must always be the case. What upon the whole appears most probable is, that a general excitement of the uterine system, not necessarily connected with the cooperation of the male, produces the *evolution* of one of the vesicles; that this is discharged into the Fallopian tube, where, upon meeting with a portion of the seminal fluid, it becomes fertilized and impregnated, and constitutes the first stage of the existence of the fœtus." (See Syst. Phys. vol. iii, pp. 30, 31, 32. Boston Edition.)

Such is the view which Dr. Bostock gives of the seat of impregnation; a view in which are united error, indecision, and obscurity, in a mixture as singular, as any perhaps contained in the records of science.

According to this representation, the female performs herself almost the entire work of generation. She alone, without the "cooperation of the male" so far matures the ovum, or rather ovarian gland, or vesicle, as to make it burst and discharge its contents. Nor is this all. Equally unassisted, she erects the Fallopian tube, expands its fimbriated extremity, directs it to the requisite spot, and receives into its open and expecting mouth the contents of the ruptured vesicle. These contents constitute the rudiments of the fœtus, which being thus safely lodged, by the unaided female herself, in one of the Fallopian tubes, are there met by a "portion of the seminal fluid, "fertilized and impregnated" by it, and thus made to "constitute the first stage of the existence of the fœtus."

As my discussion of this point will be necessarily more ample hereafter, I shall only remark, at present, that the evidence we possess of the ovarium, and not either the uterus or Fallopian tubes, being the seat of impregnation is conclusive; such as, if clearly and correctly detailed, could not be refused in a court of justice; such, in fact, as has been received as authentic, and acted on in such a tribunal; and that there exists no more satisfactory evidence of the male seminal fluid being ever detected

in the Fallopian tubes, or even in the uterus, than there does of its having been found in the stomach or gall-bladder. All the testimony that has ever been adduced to that effect is nothing better than idle rumour, originating in conjecture. One well established fact, confirmatory of the hypothesis, cannot be produced.

I well know that a few reputed facts, on this subject, are referred to. But I as well know, that, when severely scrutinized, they pass from facts into mere surmises. To rely on them, therefore, as evidence, and report them, as such, is to be unfaithful to science, and to propagate error.

Dr. Bostock's logic is as faulty as his facts. "Cases," says he, "not unfrequently occur, of perfect fœtuses having been found in the tubes, or where they had escaped from them into the cavity of the abdomen. Hence it is demonstrated that the ovum is occasionally impregnated in the tubes."

Between the Doctor's premises and conclusion, there exists here no necessary relation; nor even the shadow of it. By the discovery of "perfect fœtuses" in the Fallopian tubes or the cavity of the abdomen, it is not "demonstrated that the ovum is impregnated in the tubes." Such evidence, considered alone, renders it fully as probable, that it was impregnated in the ovarium, before its reception into the tube. When taken in connection with other well known circumstances, the evidence tends to this conclusion with a force that is irresistible.

In another clause, which I proceed to quote, the Doctor, if I am not mistaken, virtually contradicts one of his preceding opinions.

"From these considerations, says he, it will follow, that we must have recourse to that modification of the hypothesis of Epegenesis which supposes the fœtus to be formed in the first instance, by matter derived from each of its parents."

This could not, I think, have been the opinion of our author, when he alleged, that by the excitement and action of the uterine system alone, without male cooperation, the matter or rudiments of the fœtus are deposited in one of the Fallopian tubes, where it is "merely impregnated and fertilized," in other words

quickened into fœtal life, by the semen masculinum. By the terms "impregnate and fertilize," I cannot persuade myself that Dr. Bostock meant formation, by any material addition to the substance of the fœtal rudiments. From the sense in which the term "impregnate" is used by physiologists, it certainly does not admit of such an interpretation. The meaning of "fertilize" may be less definite, because, strictly speaking, it is not a physiological term. I am satisfied, however, that, by the entire expression, our author meant simply to "quicken or vivify," and thus prepare for fœtal nourishment and growth. The semen masculinum he considered as the mere leaven of fœtal life, not as a necessary addition to fœtal rudiments. The extreme minuteness of the quantity of that fluid requisite to impregnation, as ascertained by Spallanzani, and to which the Doctor refers, seems proof of this. I cannot, therefore, repress the belief, that between the two clauses which I have quoted from the "System of Physiology" there exists a contradiction. But it is of little moment whether they agree or disagree. There is good reason to believe that they are both erroneous. I shall close my remarks on Dr. Bostock's "System" by observing, that, as a book of reference, it is valuable, because it is an elaborate epitome of physiological learning. It faithfully represents what distinguished physiologists have thought and written, and is, therefore, an acceptable history of the science. But while the Doctor has benefitted the profession, by furnishing them with a very useful syllabus of the sentiments of others, he has not enhanced his own reputation, as a writer either original in his views, or independent in his opinions. He must be content with the character of a laborious compiler, and aspire to nothing higher. But to proceed in my inquiry.

As respects the knowledge of generation, it has been already stated, that what constitutes, at present, the chief point of uncertainty and difficulty, is the province of the semen masculinum. Where is that fluid deposited, *in coitu sexuum?* where does it go afterwards? and what office does it perform? Satisfactory answers to these questions would do much toward determining a controversy, which has subsisted among physiologists

since the period of the earliest records of medicine. Without presuming myself capable of framing such answers, I shall render faithfully the best I can. They will consist in part of fact, and in part of induction, amounting to the highest probability I can reach.

In the sexual embrace, we know that the male emits a fluid denominated seminal: but we do not know that the female emits any thing entitled to the same name. On the contrary, we have good reason to believe that she does not. We further know that the seminal fluid is deposited in the vagina; and there our knowledge ends. All beyond this is, at best, but supposition. We do not know that the fluid finds its way into the uterus, much less into the Fallopian tubes. It will appear presently that the notion to this effect rests on a foundation exceedingly slender. Were the semen masculinum even admitted into the uterus, it would seem almost impossible for it to enter the tubes. The reason of this will be stated hereafter.

To ascertain what is really fact, and what only conjecture, on this subject, innumerable experiments have been carefully made, by some of the ablest physiologists both of the present and former times. At periods differing from a few minutes to many hours, and including all intermediate times, after coition, the uteri of various kinds of female animals have been accurately inspected, and no semen masculinum ever detected either in them, or the Fallopian tubes. As far as I am informed on the subject, two statements only in opposition to this have ever been made. By the aid of his glasses Leeuwenhoek is said to have found, in reality or imagination, spermatic animalculæ in the uterus and Fallopian tubes of a female rabbit, and also in those of a female of the canine race. But although many other practised naturalists have repeatedly examined the organs of the same, as well as of other different animals, with glasses equally good—some of them much better—the discovery has never been confirmed. No such animalculæ, nor any thing else clearly indicative of semen masculinum have ever been detected by them beyond the vagina. And there it has been very repeatedly seen, an evidence that, wherever it exists, it is easily found. The evidence,

therefore, that Leeuwenhoek never discovered the seminal fluid either in the uterus or the Fallopian tubes, is greatly preponderant over the opposite. Besides, no one who has ever engaged in them can be ignorant of the deceptive nature of microscopical observations. To rely on them for philosophical or practical purposes, is exceedingly hazardous. Notwithstanding all that their advocates may say in their behalf, when they are pushed to great minuteness, they are much better calculated to amuse than to instruct; to excite wonder, than to disclose truth with an accuracy to be depended on. An enthusiast, as relates to the world of atoms, discovers, with his microscope, many objects, which exist only in his own fevered brain. Of this a variety of ludicrous instances might be detailed. I once myself saw a votary of the microscope, who firmly believed and most sturdily contended, that he had detected certain miniature animals, completely formed, among the ordure of their races. No wonder, then, that Leeuwenhoek, who was a believer in the preexistence of fœtal germs, and strongly prepossessed in favor of the hypothesis, that they enter the uterus to receive their nourishment and perfect their growth, fancied that he detected them in that organ. Let a thorough believer in the nocturnal visits of ghosts to places of corporeal interment, enter a churchyard by night, and the chances are numerous that a ghost will appear to him; the more especially, if he be anxiously in quest of one, and possess, at the same time, an ardent temperament. But there are yet other reasons calculated to induce a belief, that the semen masculinum produces impregnation without entering the uterus or Fallopian tubes.

That that fluid does not always and necessarily enter the uterus, *vi jaculationis penis masculini*, is certain. Impregnation has been produced in cases, where no such projection could possibly take place, in consequence of the orifice of the urethra being so misplaced, as to be on the under side of the male organ. Nor is this all. The generative power of the male is not destroyed by the truncation of the penis, even to the extent of more than two thirds of its length. To those who have industriously inquired into this subject, this statement is

known to be true. But, under such mutilation and natural imperfection, no one will contend or believe that the semen masculinum can, *in coitu*, be injected into the uterus.

Females have been impregnated, in numerous instances, in whom there existed in the vagina, os tincæ, or collum uteri, such mechanical obstructions as rendered the passage of the seminal fluid absolutely impossible. Examples of this description have fallen under the notice of competent witnesses now living; but whom I shall not name; because I have not their permission to do so. But for ample testimony of the existence of such cases, I refer to the writings of Harvey, Hildanus, Morgagni, Ruysch, Simpson, Haller, and others.

Aware of these difficulties, but still resolved on carrying the semen masculinum beyond the vagina, and mingling it, if possible, with the elements of the fœtus, as furnished by the female, certain physiologists have imagined a set of absorbent vessels, commencing in the vagina and terminating in the ovarium; or, at least, passing through it. But no such provision of absorbents exists. The hypothesis is a fabrication, imagined for the purpose of vanquishing a difficulty. Observation tells us, that the only absorbents connected with the vagina do not pass near to the ovarium.

Other physiologists, again, equally resolved to bring into contact the male and female generative portions of matter, have contended, that the semen masculinum is taken up by the common absorbents of the vagina, conveyed into the blood, and, by the circulation of that fluid, carried, in its formal and active state, into the substance of the ovarium. According to this hypothesis the entire female system is impregnated; and such is the open declaration of its advocates. Were it worthy of refutation, three objections might be urged against it.

1. No semen masculinum has ever been detected in the blood of a pregnant female, notwithstanding the experiments that have been instituted for that purpose.

2. Were that fluid to be taken up by the absorbents, its nature would be changed, and its generative efficacy destroyed, by the alterative powers of the vessels conveying it.

3. The worst is to come. Were the hypothesis true, females might be inoculated for pregnancy, as easily as for small pox. Introduce into the system of an unimpregnated female a small portion of the blood of one in a state of pregnancy, and the work would be done.

It is difficult to determine which is most striking, the silliness of this hypothesis, or the evils that would necessarily arise from it, were it true.

On the hypothesis of Dr. Bostock, and others who concur with him in the belief, that impregnation, is produced in the uterus or the Fallopian tubes, by a union there between the semen masculinum and the matter discharged from one of the ovarian vesicles, I shall offer another remark.

It is ascertained that, in the human female, nearly three weeks elapse, after an act of fruitful coition, before the contents of the vesicle of the ovarium pass along the tubes and enter the uterus. But, according to the views of Dr. Bostock, those contents are not acted on, by the semen masculinum, until this period. Hence that fluid must remain unchanged, in the uterus or Fallopian tubes, nearly twenty days,* waiting the arrival from the ovarium of the portion of matter which the female furnishes towards the formation of the fœtus. Let those who can, believe such a tale. To me it appears as incredible a fiction as Ovid's metamorphosis of Actæon into a stag, or Daphne into the laurel. For the semen masculinum to remain unchanged, during the space of forty eight hours, or perhaps half of that time, in either the uterus or the Fallopian tubes, may be pronounced impossible.

It has been already mentioned that impregnation has taken place, in many cases, where, in consequence of some mechanical obstruction, the passage of the semen masculinum into the

* I well know that, by some physiologists, a shorter period is allowed for the entrance of the contents of the ovarian vesicle into the cavity of the uterus. They are said to have been found there, in the human female, on the seventh or eighth day after coition. But proof of the fact is wanting. About the eighteenth day is, I think, the earliest period at which they have been positively found in the uterine cavity. But even seven days make a period by far too long for the semen masculinum to remain unchanged either in the uterus or the Fallopian tubes. On no ground is the hypothesis even plausible, much less tenable.

uterus and Fallopian tubes has been prevented. To this I shall add my positive belief, that such passage is impracticable at all times, even in the most healthy and natural condition of the parts. This belief I rest on evidence derived from the nature and consistency of the semen masculinum, and from the mechanical structure and relations, with the condition and powers of the male and female organs of generation. When the combined influence of all these particulars is weighed, but on the discussion of which I cannot dwell, I repeat, that it does not seem possible for the seminal fluid to find its way into the uterus and tubes.

Another hypothesis, worthy of notice, is, that the semen masculinum not only finds its way into the Fallopian tubes, but is conveyed by them to the ovarium, where, by its presence and immediate action, it impregnates one or more of the ovarian glands.

The refutation of this is easy. In their flaccid condition the Fallopian tubes cannot convey any thing from the uterus to the ovarium. To be fitted for such a function they must be excited to a state of turgidity, or vital erection. But, from the experiments of Dr. Haighton, it clearly appears, that into that state they are never awakened, until after impregnation has taken place. The object of their erection *then* is, not to convey any thing from the uterus to the ovarium, but the reverse. It is to take up the contents of the matured and ruptured vesicle, the real rudiments of the future embryo, and deposit them in the uterus. This part of the generative process will be more fully described hereafter.

Such are the sundry and earnest efforts which physiologists have made, to bring together, in some shape and situation—in the uterus, the tubes, or the ovarium—the semen masculinum and the rudiments of the fœtus furnished by the female. But they have made them to no purpose. Between the two specified portions of matter, nature has placed an impassable barrier. They cannot come together. Nor is it requisite that they should. The process of generation can be effected without their meeting as certainly, and as well, as with it.

What facility, in explaining generation, would the coming together of those two portions of matter give to physiologists, who so zealously contend for it? I answer none; and no one, acquainted with the subject, will question, for a moment, the correctness of the reply.

I well know that the chief object of solicitude, on this and other analogous subjects, is to bring things together, and place them in mutual contact. But I as well know that the anxiety is without any positive meaning, and the effort, should it even succeed, without the anticipated effect. Inquirers into the mode of operation of agents on living matter gain nothing by bringing the agents into contact with the parts on which they act. No aid, in the way of explanation, is thus acquired. The precise cause and manner of living action, excited by proximate impression, are no better understood, than those of action resulting from impression which is remote. Indeed so completely is the system unitized, by means of sympathy, that we are scarcely authorized to use the phrase "remote impression." All impression might be almost pronounced proximate, as relates to every part of the body; because sympathy brings all parts, even the most mechanically remote, into vital contact. When, in a case of pleuritis, a blister is applied to the thorax, we know just as well why and how it acts on the pleura, as why and how it acts on the skin. When a dose of tartar emetic is swallowed, we can explain as satisfactorily its action on the skin, as its action on the stomach. And when an excessive quantity of tincture of cantharides is taken into the stomach, its mode of operation on the urinary organs is as well understood, as its mode of action on the organ to which it is immediately applied. Of all other remediate agents the same is true. And yet, I repeat, physiologists and pathologists, for the sake of fancied explanation, have an unconquerable passion, for bringing all agents into immediate contact with the part on which they operate. Does nitre, fox-glove, and uva ursi act on the kidneys? They must, according to this "contact"-hypothesis, mingle with the blood, and in their formal character, reach those organs, by the circulation of that fluid. Is the muscular system invigorated by a course

of chalybeates? The mineral must, in like manner, enter, the blood, and, with its qualities unchanged, penetrate the muscles, and every other organ, whose action it strengthens. It must even penetrate the brain, and become a part of it; in as much as the strength of other portions of the system is essentially connected with the condition of that organ. Do ardent spirits produce intoxication, by their influence on the brain? By absorption or otherwise, with all their spirituous qualities unaltered, they must find their way to that organ, and immediately stimulate it. Or does the matter of small pox, by being inserted under the cuticle, produce a disease of the whole system, and pustulous eruptions on different parts of it? It can do this only by mingling with the blood, and converting the whole of it into an empoisoned mass. Such, I say, is the prevalent doctrine, and such the disposition of many to bring every agent, whether remediate or noxious, to the very point on which it operates. A notion more unfounded and visionary never shed its influence on the mind of a dreamer.

That it may produce its full effect on living matter, I repeat with confidence, that it is not requisite for an agent to be brought into contact with the organ on which it acts. I will add, that it is not requisite for it to be brought near to it. As has been already several times asserted, in the production of excitement, remote impression is as powerful as proximate. Of this, innumerable instances can be adduced, and its explanation is to be found in the laws of sympathy.

Do we wish to act on the liver, and produce or augment the secretion of bile? We make, by calomel, or tartarized antimony, an impression on the stomach and alimentary canal. No particle of the medicine touches the liver. Do we wish to excite the skin, so as to produce or increase secretion by that organ? We again impress the stomach by tartar emetic or ipecacuana. Is it our desire to augment secretion by the kidneys? We apply cold to the skin, or excite the stomach by a diuretic agent. The emotion of fear, a passion seated not in the kidneys but the brain, will do the same. The warm bath, acting on the skin, excites the secretions of the alimentary canal, and

other abdominal organs, as well as of the mucous lining of the lungs. And who does not know, from personal experience, that, when the system is excited by warmth or exercise, a draught of cold water, acting on the stomach, proves often a powerful sudorific? The facts, to the same effect, that might be adduced, are innumerable. In no instance do we act immediately on the organ, whose secretion we would promote. And if we were thus to act, we would fail in our purpose. For, to all accurate observers it is perfectly known, that an impression which, when made primarily and immediately on a gland, does not increase its secretory action, produces that effect with great certainty, when made on a distant organ. An injury done to the brain is the cause of a copious secretion of bile; while a similar injury to the liver itself prevents the secretion. A pleasant impression made on the brain by the sight of grateful and desirable food, augments the secretion of saliva; and a painful impression of another description made on the same organ excites the lachrymal glands, and gives rise to a flow of tears.

Nor is it secretory action alone that is thus promoted by distant impression. The same is true of every kind of living action, whether healthful or morbid. Does a patient labour under arthropuosis, and is the effusion seated in the knee-joint? Tartar emetic acting on the stomach excites absorption and cures the complaint. But will that remedy have the same effect, if it be introduced immediately into the diseased joint? We know it will not. On the contrary, it will aggravate the existing malady, or produce a new one. Another patient is affected with a catarrhal irritation of the lungs, which is mitigated or removed by a dose of laudanum brought into contact with the stomach. Would the complaint be equally weakened, or cured, by applying the remedy immediately to the organ in which it is seated? It certainly would not. It would be rendered worse. Cutaneous eruptions, and some forms of ophthalmia are most effectually remedied by impressions made on the chylopoietic organs. Make similar impressions immediately on the eyes and skin, and mischief will be the issue.

But why should I thus proceed in details? Nineteen twen-

tieths, and perhaps even a larger proportion, of all the complaints to which man is subject, are excited in some of his organs by distant impressions, and cured in the same way. There are but few diseases, in the treatment of which the remedies are applied to their immediate seat. We practice medicine by remote impression, which produces the effects we aim at, by means of sympathy. As a majority of complaints have their seats in internal and inaccessible organs, if we could not thus practice, medicine would be in a high degree defective and inefficient. Such is the doctrine, which I believe to be sound. To apply it to the solution of some of the phenomena of generation shall be my next object.

Generation has been already pronounced a secretory process. It is as really and obviously so as the formation of bile, saliva, or tears. There are concerned in it, and alike essential to it, two distinct processes of secretion. Of these one pertains to the male, and the other to the female. The former produces the seminal fluid, in the male testes, the latter the true rudiments of the fœtus, in a vesicle or small gland of the female ovarium. In this joint, or rather consecutive process, the priority of action belongs to the male. By its specific property, as a stimulus, the semen masculinum excites to specific and suitable action the small ovarian gland, and the issue is the fœtal rudiments, prepared to take on the further kind of action requisite to the formation and evolution of a fœtus. This effect it produces by remote impression, without coming into contact with the gland which it excites. That I may be clearly understood, I must speak on this subject more explicitly and fully.

The female has two ovaria, in each of which is situated a number of small vesicles possessing the nature of glands. Of these glands the peculiar function is to secrete from the blood the rudiments of a fœtus. But this they cannot do under common excitement; I mean the excitement of ordinary stimulants. There is in nature but one stimulus that can awaken in them the specific action of the generative process. That is the semen masculinum. But it has been already shown that this cannot be brought into contact with them. It acts on them,

therefore, by remote impression, through the medium of sympathy. The more to facilitate its operation, it will be borne in mind, that, as already stated, the sympathetic connexion between the various parts of the female generative system is very lively and strong. In the process of generation, the following appears to be the order of events.

The semen masculinum is deposited in the vagina fitted to receive it.* The specific impression which it produces on that organ is the only primitive and idiopathic act in the generative process. Every subsequent one is derivative and sympathetic. The first sympathetic movement is in one or more of the ovarian vesicles. By the peculiar stimulation of the seminal fluid on the surface of the vagina, those vesicles are excited to that specific kind of secretory action fitted to produce the fœtal rudiments. The change they undergo is striking. From having been pellucid, both in their covering and contents, they become thickened, turbid, and injected with blood. Their size is augmented, and their progressive change resembles that of a pimple or small tumor passing to suppuration. The vesicle being matured, and its covering thinned by the action of the absorbents, it at length bursts and discharges its contents into the Fallopian tube, which, by a second act of sympathy, is prepared to receive it.

This act of preparation had been going on for some time, previously to the bursting of the ovarian gland, and was the result of the excited condition of that gland. Its phenomena are peculiar. The parietes of the tube become injected with blood, its caliber is enlarged, from a flaccid and depending condition it becomes more rigid and erect, its fimbriated extremity, in a particular manner, expands, the fimbriæ are applied to the ma-

* It has been made a question, whether, to effect impregnation, it be essential that the female feel desire, and experience gratification in coitu? It is not. I think it probable that a certain degree of these renders impregnation more certain. But it is not essential to it. Many females have borne issue, who not only experienced no desire or gratification, during coition, but who had even a repugnance to the act. In proof of this, facts in abundance might be adduced. And it is well known that those females whose desires are strongest, are by no means the most prolific. Something much nearer the reverse is true.

tured and bursting vesicle, they receive its contents, the real rudiments of the fœtus, and the tube conveys them into the uterus. Having completed its function, the tube returns to its flaccid and inactive condition.

But the chain of sympathetic movement is not yet complete. To receive and accommodate the fœtal rudiments, the uterus undergoes the necessary preparation. This is effected chiefly by a twofold change; one, at the fundus, and the other at the collum uteri. The object of the former is to afford to the fœtal rudiments, now called an ovum, a place of secure attachment to the uterus, and that of the latter, so to close the neck of that organ, that the ovum cannot escape from its cavity.* The medium of attachment to the fundus uteri becomes the placenta, and the passage through the neck is obstructed by a mucous secretion of the requisite viscidity.

Two further sympathetic movements remain to be mentioned, and the circle is finished. These are the changes that take place in the external organs of generation, preparatory to parturition, and in the mammæ, preparatory to the secretion of milk. The nature of these preparations being known to every medical observer, need not be specified. They are precisely such as wisdom would dictate, and aptitude requires. The mammæ are enlarged, and the external organs rendered more dilatable.

This entire circle of changes, with others that appear in remoter organs, and in the general system, belong essentially to the generative process, and constitute, each of them, a part of it. The original impression made by the semen masculinum on the vagina excepted, they are all sympathetic. None of them but that is produced by the presence and immediate action of the seminal fluid. Nor is this contended for, I believe, except as relates to the change produced in the state and secre-

* Some physiologists contend, that the object of nature, in closing the collum uteri, is to exclude the semen masculinum, in coition during gestation, and thus prevent superfœtation. But the notion is unfounded. It is conceived moreover without reflection In female quadrupeds, that do not admit the male, during gestation, the collum uteri is as completely closed, as it is in woman. After conception, the altered condition of the female generative system is sufficient to guard against further impregnation. The object then, I say, of closing the uterus, is to prevent the escape of the ovum which it contains.

tion of the ovarian vesicle. That being the real act of impregnation, without which the acts that follow would either not occur, or be of no avail, the formal presence and direct operations of the semen masculinum are supposed to be essential to its production. It is conceded and freely concurred in, that all the other and subsequent changes, in the Fallopian tubes, the fundus and collum uteri, the external organs of generation, and the mammæ, may be sympathetic. But that in the ovarian gland, say my opponents, must be produced by the immediate presence and primitive action of the seminal fluid.

But why is this? On what ground of reason is the assertion made? We have seen that, as relates to other glands, remote impression is as powerful in exciting secretory action as proximate. Why, then, should not the same be true as respects the ovarian? The change produced in that gland, in the process of generation, is not greater than that which occurs in the tubes and uterus, but more especially in the mammæ. Yet no one contends, or has ever contended, that the semen masculinum must be conveyed to the mammæ, in order to produce the secretion of milk. Why, then, is its presence in the ovarium more necessary, to produce there the secretion of the fœtal rudiments? Let those who can answer reasonably this interrogatory, do it. I cannot. Nor do I believe it can be so answered by any one. As the mammæ are acknowledged to secrete milk by sympathetic excitement, so may the ovarian vesicle, by such excitement, secrete the rudiments of the fœtus. So in fact does it; for it experiences no other kind of excitement.

For one thing, in particular, I admire my antagonists; their courage and intrepidity. They boldly encounter two difficulties to my one. Theirs are, the difficulty of getting the semen masculinum to the Fallopian tubes and the ovarium, and the undiminished difficulty of explaining its action when there. I, on the contrary, have but one difficulty to meet; that of accounting for the impregnation of the ovarian vesicle, by remote stimulation. And that, I repeat, is no greater, than to account for the same by proximate stimulation. To accomplish their object, therefore, the amount of their labour and difficulty

must double that of mine. This unquestionably diminishes the probability of their success.

One remark further. Let the opponents of the sympathetic theory of generation open the abdomen of a female animal, and apply fresh semen masculinum to an ovarian vesicle, and mark the result. Instead of the secretion of the rudiments of a fœtus, inflammation will ensue. Why? Because the male seminal fluid is an *irritant* to the female ovarium, when brought into contact with it. Its stimulating power is out of harmony with the susceptibility of that organ; as much so, as the stimulating power of the bile is out of harmony with the susceptibility of the peritoneum, which we know it will inflame; and for the same reason. The semen masculinum acts naturally and harmoniously on the vagina, but on no other part of the female system. This experiment I have never performed. But, as far as all analogy may avail, the result would be as here stated.

If I am not greatly mistaken, this view of generation simplifies it not a little, and renders it more intelligible than any other. It takes from it much of the mysteriousness attempted to be thrown around it, by showing that, instead of being a process entirely insulated, it rests on the same principles and laws which govern the other functions of living matter. It shows it to be especially the result of stimulation, sympathy, vital erection, secretion, absorption, and contraction, the latter being in part organic and insensible, and partly animal and perceptible. And by these modes of action are performed also nearly all the functions of living matter, whether healthy or diseased. The sympathetic theory of generation, then, is natural; because, in its principles, it is identified with digestion, nutrition, respiration, and other processes both animal and vegetable. But those theories to which it is opposed are unnatural, because they have very little, if any thing at all, in common with the established and well known operations which constitute the economy of animals and vegetables. In this point of view, then, the sympathetic theory has a decided superiority in its claim to belief. To pronounce it natural is to pronounce it true. For nature

and truth are identical. Whatever therefore differs from or is opposed to nature is necessarily false.

Were I inclined, in this inquiry, to avail myself of the resources of analogy, it would open to me not only a wide and attractive, but an appropriate and opulent field of illustration, at least, if not of proof. As far as man has been able to penetrate and comprehend the general economy of things, nature works on a plan of great uniformity. A vast and sublime analogy pervades her operations.

But that vegetable impregnation is effected by sympathy cannot, I think, be doubted. Many plants might be mentioned, in which the ovarium, or pericarp, containing the rudiments of future seeds, is as rigorously withheld from all immediate access of the farina, or male fecundating dust, as if it were inclosed in glass hermetically sealed. Yet impregnation takes place in these cases, as certainly and regularly, as in any others. Sympathy then must be the channel.

As relates to many tribes of inferior animals the same is true. Even in the mode of coition and impregnation of our common domestic fowls, it is, if not impossible, in the highest degree incredible, that the male semen ever makes its way to the female ovarium. My individual conviction to this effect is the more complete, from the consideration, that, in a single act of copulation, the male can fecundate every egg, frequently more than twenty in number, which the female is to produce during a whole season. Let the act take place at the commencement of the season. At this period, although a few of the ovula in the ovarium are somewhat developed, a great proportion of them are so exceedingly small that they are scarcely visible. In this state of things, the minute ovula, being situated behind and under the larger ones, the passage to them is so completely obstructed, as absolutely to prohibit the access of the semen masculinum. Add to this, that the matrix and infundibulum, in the domestic fowl, make a long, rugous, winding canal, and that, to come into contact with the ovula, the semen must pass along its entire length; for that fluid is simply deposited, *in coitu*, in the vulva, or rather cloaca, situated at the lower end of

the canal, while the ovarium is seated at its upper end. The semen masculinum, moreover, being a viscous fluid, both in fowls, quadrupeds, and the human race, and the surface along which it has to travel, being lined with a fluid equally viscous, its chance to make headway is about as bad as can be imagined. The thick and glutinous nature of the semen masculinum is, in a particular manner, a reason why it cannot pass through the almost impervious neck of the Fallopian tubes, in the human subject. It furnishes also a strong objection to its entering the uterus, during coition, *vi jaculationis*. When all these circumstances are taken into view, the notion that the seminal fluid reaches the ovarium seems as visionary, as any tale that fiction presents. In wildness and incredibility, oriental fable scarcely surpasses it.

One fact more on this topic, and I am done. In the aphides, a small troublesome insect, that infests our cabbage in myriads, a single act of coition, by the male, fœcundates, not a single female, but several successive generations of them—mother, daughter, grand-daughter, great-grand-daughter, and others still remoter in descent. But to allege that each female of the line receives into her system, and brings into contact with her ovarium, a portion of the semen of the common and remote father of the whole, would be to advocate a notion too absurd to be refuted. The fact being a very curious one, in the history of animal propagation, deserves to be stated more circumstantially.

The aphides, of which I am speaking, are both oviparous and viviparous. They are oviparous once in the autumn, and viviparous through several generations in the summer. In the latter end of September, the females (there are no males of them then living) deposit their eggs in suitable situations, and soon afterwards die. Those eggs remain inactive throughout the winter, but produce, on the return of the vernal warmth, a progeny consisting of males and females. When arrived at maturity, the sexes unite, the males impregnate the females, and soon afterwards expire. The progeny that succeeds is wholly female. But the same embrace which impregnated

their mothers, fœcundated them. They give birth to another race of daughters, that are also fœcundated from the same source. This race brings forth another, and that another, all being females, as before, until the September birth, which is again one of eggs. The eggs, in spring, produce males and females, who, as already stated, celebrate their loves, and thus is the species continued. No one, I repeat, will contend, that, in this mode of propagation, each succeeding progeny of females is impregnated by the presence and immediate action of the seminal fluid of their common but distant sires on their ovaries. The notion would be too silly to gain a single advocate.

Shall I be told that the sympathetic theory of generation is unfavourable to the propagation of paternal and mixed likenesses? and that, if it were true, no likeness could be communicated from parent to offspring, but that of the mother? I reply, that it is just as favourable to the handing down of paternal, and the production of mixed likenesses, and explains them as well as any other theory. We can understand just as well how the peculiar stimulus of the seminal fluid of an individual, or the peculiar impression made by it on the vagina, can imprint the likeness of that individual on his offspring, as how that likeness can be imparted by the peculiar size, shape, affinities, or other mechanical or chemical properties of the particles of that fluid. In truth we know nothing of the matter. In the present state of natural science, the transmission of parental likeness, and the production of hybrids, are points of transcendentalism, respecting which it is much wiser in us to confess our ignorance, than expose it, by an unsuccessful effort to shed light on them. Some of the most empty and unintelligible speculations, that have issued from the press, have been on this subject. Until fact shall have been substituted for hypothesis, they ought to be abandoned. At least the topic to which they relate should never be made a ground of belief or disbelief as respects any thing else.

The following curious but authentic facts show how completely the production of hybrids sets at defiance our attempts to explain it.

It is known to naturalists that of the genus equus, or horse family, there are five species, the equus caballus, or common horse, the equus asinus, or ass, the equus zebra, the equus quaga and the equus jiggetai. Of these the asinus, zebra, quaga, and jiggetai, are much smaller than the caballus, and, in their general appearance and character, differ widely from him, and from each other.

In the year 1818, a gentleman, living in the neighbourhood of Edinburgh, became possessed of a young Arabian mare, and a male quaga. Being kept in the same stable, the two animals came together, by the negligence of the hostler, and the consequence was the impregnation of the female. At the proper time the offspring appeared, and was a well marked hybrid, partly quaga, and in part caballus. After the production of the colt the two parent animals were never permitted to be again together.

Not pleased with such a mongrel breed, the proprietor of the mare sent her, in the spring of 1819, to a fine Arabian horse, that stood in the vicinity of London. She was impregnated by him, and had another colt at the usual period. But, to the surprise of every one, it also was a hybrid, the characteristic marks of the quaga being as evident and strong in it, as they were in the preceding one.

In the spring of 1820 the mare stood again to the same Arabian horse. The connexion proved fruitful, and in 1821 the issue was a third hybrid colt, the marks of the quaga being again as prominent, as they were in the other two. The birth of the third colt had taken place a short time before my arrival in London, in that year, and was a topic of much conversation, and no little speculation, among the physiologists and virtuosi of the metropolis*.

In the Hunterian museum were suspended accurate likenesses of the three parents, and their three descendants, the male

* To remove all suspicion of a furtive intercourse between the quaga and the mare, the reader is assured, that from the spring of 1819 until the period of the birth of the third colt, they had not been nearer to each other than the neighbourhood of Edinburgh is to that of London. During that whole time the male was in the former place, and the female in the latter.

quaga, the Arabian horse and mare, and the three caballo-quaga colts. There was also, in the museum, an authentic record of the facts. I saw the likenesses, read the record, and received, in conversation, from the keeper of the institution, a still more circumstantial account of the occurrence. Dr. Good refers to it briefly in his "Book of Nature."

The only remark I shall make on this extraordinary case is, that it appears to me to exhibit, in a stronger light than any other I have ever known, the paramount influence of the female mind in the transmission of parental likeness.

I have said that the rudiments of the fœtus are the result of glandular secretion. Shall I be asked whether they are secreted in the fœtal or embryotic form? and if not, at what period, and by what power and mode of operation they are made to assume it?

I reply, that I feel persuaded they are not secreted in the fœtal or embryotic form; but at what time, or by the influence of what causes, they assume or receive that form, I cannot tell. Might I, however, hazard an opinion on a subject so hidden (did I not dislike the term I might have said "mysterious") I would say, that they are not moulded by external influences, but that they assume the form by a vital, inherent, plastic power of their own. I apprehend that in the human race, and in all quadrupeds that propagate their species in the same way, their is but little difference of appearance in the fœtal rudiments, when first secreted. They seem to constitute, in the words of the poet, "*rudis indigestaque moles*," a mere gelatinous or semi-mucous mass, destitute of all specific form. It is perhaps perfectly certain, that no physiologist or naturalist can distinguish, by inspection, the early fœtal rudiments of a human being from those of a horse, except that possibly the latter may be the largest. Nor do those rudimental masses receive their organization and specific forms from any influence of the Fallopian tubes or the uterus. As already mentioned, each mass appears to be endowed with a native propensity and power of its own, to assume the figure and appearance of that species, by which it is produced, and of which it is destined to become an indi-

vidual. This figure it puts on by a living and peculiar effort, that has been technically denominated "Nisus formativus." Nor am I inclined to concur with certain writers in condemnation of this phrase. It assumes the existence of no new and distinct principle, but simply expresses what appears to be a fact, the result of principles already recognized. It represents, that a mass of living matter, as yet formless, assumes the form it is destined to wear, not in obedience to any influence from without, but by the exercise of inherent powers of its own. And to me the representation has the semblance of truth, because nature appears to sanction it. Although I have expressed a belief, that no physiologist or naturalist can distinguish, when they are first secreted, the fœtal rudiments of a human being from those of a quadruped, I do not, by this, mean, that they are, in all respects, the same. In their plastic powers they must differ, otherwise they could not put on different forms. But their possessing different powers cannot be discovered by a mere examination of them.

One of the most universal attributes of living matter, which presents itself to us at all times, from all quarters, and under all circumstances, is the immutable propensity of every individual to assume, maintain, and restore, if accidentally lost, the form of the species to which it belongs. It pertains to the vegetable no less than to the animal kingdom, and to the humbler no less than to the higher orders of animated nature—to the lichen that creeps, in common with the oak that towers, and to the lowly worm, in common with man the chef d' œuvre of earth. It is, therefore, a principle of creation, whose influence contributed to the expulsion of chaos, by calling order out of confusion; and which will last and operate until chaos shall return. In simpler language, it is a law of nature, which every one perceives and recognises, but which no one understands. And its universality and permanence are the reasons why it is so little noticed, admired, and spoken of.

The fœtal rudiments of the oak, the Adansonia, and the plane-tree, are, in substance and form, identical with those of the walnut, the cedar, and the hemlock. At least the severest

scrutiny can discover no difference between them. Yet nothing earthly can convert the former into the latter. The decree has gone forth, that each be configured after its kind, and HE only who issued it is able to annul it.

Of the animal kingdom, as already intimated, the same is true. Man is so far literally akin to the worm, that originally their rudiments may not be distinguished. Both are crude, shapeless, and unsightly. But by virtue of the inherent attributes of each, the one grovels, the simplest and meanest of forms, while the other shoots upward, the noblest and most multiplex.

Yet in all this, I say, there is nothing of external influence or moulding. It proceeds not from without but from within. It is as much a part of the nature of the substance concerned, as its weight, consistence or colour. Remove it, and the substance is as really changed in its nature, as if it were converted into iron or brass. This formative attribute is to living animal and vegetable substances tantamount to what gravitation is to the material universe, as composed of globes, and the laws of crystallization to mineral bodies—an inherent property which gives them their organization and shape. It is, therefore, as natural and permanent as attraction, and as little understood.

It is by means of this "Nisus formativus," this form-bestowing-preserving-and-restoring power, that a polypus bisected converts itself into two individuals of its kind, that a decapitated snail reproduces its head, a mutilated crab, the member it has lost, a water-newt, its exsected eye, and an arborescent plant, the limbs that have been lopt off. It is owing to the same power, that, in whatever position a seed may be planted in the earth, its plumule shoots upward, and its radix downward. Nor is it attributable to any thing else, that even the human system repairs its mutilations. If, by mechanical violence, a portion of muscle is removed, a new growth is excited, and muscle is restored. If a bone is broken, or a fragment of it carried away, the injury is repaired by a reproduction of the same substance. Does the mutilation consist in a loss of membranes, tendons, or even nerves? The portions taken away are

reproduced, in time, by an appropriate growth. Nor does this new growth ever so far mistake its province, or miss its aim, as to reproduce one kind of tissue or organ, to repair an injury sustained by another. If a piece of muscle be carried away, it never fills up the chasm with bone, nor the reverse; nor does it put down a tendon or a muscle where a nerve has existed. Unless prevented by disease or otherwise, it replaces lost parts according to their kind. All this it does without moulds to cast in, or models to work by. The atmosphere does not act as a mould to give shape to the leaves, branches, and stems of vegetables, nor the earth to their roots. Nor do the Fallopian tubes or the uterus bestow figure on the embryo they transmit and contain.

I know it is contended by many, that the germs of all the parts of animals and vegetables, restored by a new growth, preexisted, in miniature but perfect forms, in the systems of those beings; and that the restoration is nothing but a simple evolution.

This hypothesis refutes itself by its wildness and extravagance; as does every other founded on a belief in formal preexistences. To what period of time does the preexistence extend?—To the origin of the animal and vegetable kingdoms?—Did the first vegetables and the first animals of the several species created contain, in themselves, the formal germs of all their descendants, and of all the parts besides, of which those descendants might be deprived? If such be the hypothesis, I say, its extravagance renders it unworthy of a reply.

If, on the contrary, the preexistence reaches only to the birth of each individual animal and vegetable, nothing is gained by it. Each animal and vegetable must then, by its inherent powers and energies, form its own germs; and it would seem wiser that, in doing this, it should form them only as they are wanted, than that it should provide stores of them, which may never be wanted. The hypothesis in whatever light it may be viewed, is eminently absurd.

Such is the power which gives to the fœtal rudiments their intended form. It enters, therefore, as a component part, into

that exquisite and magnificent system of aptitudes, which pervades creation, from the highest to the lowest condition of being, proclaiming the existence of a God and his attributes. Which retains in their places and proper spheres of action, suns and their planets, regulates and directs to their destined points the seemingly more irregular movements of the comet, gives to worlds their most suitable configuration, and, on this earth, shapes animals according to their endowments, and endows them in harmony with the places where they reside, and the courses of life they are intended to pursue.

Such are some of my thoughts on generation. Many points of interest, connected with the subject, I have not touched; my object having been to state what is requisite to the clear understanding of the *sympathetic* theory, and nothing more. So far, I trust, I have been successful. Whether I have advanced, in behalf of that theory, any arguments new or satisfactory, the public will judge; and, in the end, they will judge correctly.

It was my intention, as announced in an early stage of this inquiry, to hazard a few thoughts on "spontaneous" generation. But my memoir having extended already to a length not anticipated, I must postpone, until a future occasion, the consideration of that curious and interesting subject.

ART. III. *Some account of the Lunatic Asylum of Kentucky, with Remarks, &c.* By SAMUEL THEOBALD M. D. late attending Physician of the Institution.

(CONCLUDED FROM VOL. II. P. 511.)

IN our former communication on the subject of this institution, we presented substantially the several acts of the Legislature in regard to it; a description of the situation and extent of the buildings; a short statistical statement, and a brief contrast of the former and present mode of providing for the insane. The further account of it which we propose to give on the present occasion, will embrace a more detailed history of its administra-

tion generally than we had at first contemplated presenting to the public.

The period has been when the dictates of charity and humanity seem to have been little regarded in the management of institutions of this character. When the unfortunate and wretched victim of insanity, whose condition calls aloud for commiseration, and for the exercise of all the kind and charitable feelings of our nature, seems to have been looked on and treated rather as a culprit who had forfeited all claims to the courtesies or regard of society and was doomed to the most loathsome and cruel confinement, and subjected to the most barbarous and inhuman treatment. But we feel much satisfaction in the assurance that, although this cruel and degrading system of management still blackens the annals of some of the institutions in Europe, it has been reformed to a very great extent, and that so far as we are informed as to the internal police and management of institutions of this kind in the United States particularly, they are conducted on the most humane and enlightened principles.

The cruel and shocking management of some of the receptacles for the insane in North Holland, is most strikingly represented in a publication made a few years since by M. Guislian, a physician at Ghent, who was called particularly to examine and report the condition of these institutions; in which he remarks, "it is painful but just to say that interested motives only guide the chiefs of these establishments. To satisfy their wretched avarice they deprive the poor madman of his necessaries, they adulterate his food, they diminish his allowance, his garments are perfect rags, and between his winter and summer clothing there is no difference. The madman is laid on some sprinklings of rotten straw beside his excrements; and to this you must add the most brutal treatment, in which chains, stripes and blows are marks of tenderness, compared with the chilling damps of their abodes and the revolting indifference of their attendants; and you have not a perfect picture but an incomplete sketch of the sufferings of men in these dens of desolation." Happily for the cause of humanity and for the reputation of our own

country in particular this sketch does not in any degree apply to our institutions.

A proper classification of the inmates of the Kentucky Asylum is duly regarded. There may be said to be three divisions or classes of each sex. The first classes are composed of idiots or of such lunatics as have become partially or entirely fatuitous or idiotic, and who pay very little regard at all to their own condition in any respect whatever. In this institution, as might reasonably be expected, this has always formed a numerous class. The second is composed of such as are capable of attending in some measure to their own wants and necessities, and have still some sense of decency and propriety of conduct, but are yet required to be restricted for the most part to narrow limits. The third class is formed of those who are so far convalescent and restored to sanity as to be employed in the domestic duties of the house, and to be allowed unreserved intercourse with the family of the keeper, and the liberty of exercise in the front yard and garden.

One division of the enclosed yard, with a large and commodious day room opening into it from the basement story, is appropriated to the first class of males. Another division of the yards is appropriated to the accommodation of the second class, who have also allotted them two large and well finished rooms; one as a day room, the other as an eating room. The third division of the yards is appropriated to the accommodation of the females, who are also supplied with a spacious and comfortable day room and eating room. An inconvenience, which ought to be obviated in the construction of buildings of this character, is here presented in these day rooms having no direct and convenient opening into the yards appropriated to the exercise and accommodation of the classes confined in them. The two first classes of both sexes have two meals a day furnished them, breakfast and dinner, both of which are served rather later than the usual time of those meals in private families. Breakfast consists chiefly of well baked corn and wheat bread, with butter and good coffee or milk, and frequently molasses. Dinner is constituted of meat which is usually boiled, but chiefly of soup, with good vegetables and bread. The eating rooms are provi-

ded with appropriate tables and benches, at which all are regularly seated, having a reasonable allowance set before them; and are superintended during their meals by the keeper and matron or their assistants. It is rare that any indecency or impropriety of conduct occurs at table. On the contrary quietness and decorum usually mark the deportment of all.

The appetites of the insane are generally on the extremes of voracity or entire aversion to food, and the latter extreme is too often the result of excess in eating when proper attention is not bestowed in guarding against it. It is certainly a subject of much importance properly to regulate their diet both in quantity and kind. No doubt can be entertained but that their health and comfort is always promoted by a due attention to this subject. The number of meals and the kind and preparation of food, we think, are well regulated in this institution; but we very much apprehend that a due attention to the quantity allowed is not always sufficiently regarded. Those who are engaged in active employment about the house and grounds may, as a general rule, be allowed a liberal if not unrestrained indulgence in diet, but those who are entirely inactive or confined to their rooms require to be carefully noticed.

It has always been the case in this institution, as in most others, that many of the insane are capable of labouring. A proper regard for economy would alone be a sufficient inducement to require the services of all such to the extent of their ability, but a consideration of still higher moment is the decided and important benefit that results to the insane themselves from regular exercise and employment. This has been observed in every institution of this kind where any opportunity of experiment has been afforded in relation to it. It is a subject on which too much stress cannot be laid. In our own institution, although it has always been regarded as of high importance, their still remains much to be done, in order to effect the fullest enjoyment of the benefits to be expected from it. There are a number both of males and females who are regularly employed in the domestic avocations of the house and grounds. Indeed the largest proportion of the drudgery and labour, required

about the institution, is performed by the insane; but the deficiency consists in not having sufficient regular and appropriate employment for all who are capable of manual labour. It is manifestly observable that the general corporeal health of those who are thus employed is generally good—instances of mortality among them are comparatively few, and a most gratifying and sensible improvement is often observable in the condition of their minds. Scarcely a case has occurred within my recollection where the individual thus employed has retrograded in his rational powers.

We are fully aware that, in regard to many patients who have sufficient bodily strength to labour, a great and almost insuperable difficulty presents itself in the danger of placing an instrument of husbandry or mechanism in their hands; for there are many who, although generally peaceful and governable, are subject to sudden paroxysms of excitement and rage, in which it would be extremely dangerous that they should have any instrument in their hands, with which injury could be done to those within their reach. In regard to such, however, it would seem to us practicable to devise some plan of employment and labour, or at least of regular exercise, free from this objection. The limited extent of land owned by this institution, at present, presents some difficulty to the employment of all who are capable and might be safely and beneficially engaged in the cultivation of a farm. But this inconvenience might be obviated, to some extent at least, by laying out and improving handsomely, as a garden, the whole of the ground allotted to cultivation. In this condition it would call for a much greater amount of labour than as at present managed, and would, at the same time, present interesting and attractive objects, and pleasant walks to such as might be capable of their enjoyment. As we have before suggested, the primary object in most cases should be to afford such employment as will call for the due exercise of the body, with a view directly to the preservation and improvement of the general health. It is known to be a matter of importance to adapt the employment as nearly as practicable to the capacity of the individual, regarding that as most useful in its influence

on the mind which demands somewhat of regular attention and judgment in its execution. The introduction of appropriate amusements with a like view is a subject worthy of serious regard, and has been to a limited extent brought into operation in this institution.

In the dress of the inmates, decency and comfort are duly regarded. In summer they are clad in domestic cottons and linen, and in winter the external dress of the males, is coloured domestic jeans of wool and cotton, and that of the females plaid and plain coloured linsey of the same materials. Both sexes are supplied with shoes and woollen stockings or socks.

They are with few exceptions separately lodged in neat and well finished small rooms, so constructed as to admit of free ventilation, and supplied with heat, as heretofore suggested, through flues passing from the basement story and opening into each room. Much care is exercised in preserving the dormitories clean and neat, in the free admission of pure air, when the season allows it, and adapting the bedding to the season and to the condition of the individuals.

The commissioners hold regular semi-monthly meetings at the Asylum, for the two fold purpose of transacting all business in relation to its civil concerns, and of giving that oversight and attention to the condition of the house and of its inmates which their duty and a just sense of humanity call for.

Although it might be inferred from the act of the Legislature defining the powers of the commissioners that they were authorized to decide, without the intervention of a jury, on all cases presented for admission, yet such is not the fact, but it is required in every case that the individual shall have undergone examination, by a jury on a writ of enquiry of lunacy or idiocy, in the county where he or she resides, or may have been found. Had the Legislature intended by that act to confer such power on the commissioners, they could not satisfactorily have exercised it, inasmuch as a previous law of the state in relation to persons of unsound mind, requires not only that the state of mind shall be inquired of, but that the condition of the individual as to property shall also be ascertained, which, in most cases, could

only be satisfactorily done in the neighbourhood or county where the individual resides. The managers have in a few instances admitted persons of unsound mind, as boarders on the application of their friends, but they have on several occasions experienced inconvenience and trouble from this course, and have established it as a rule not to admit any without the intervention of a jury. A want of information on this subject, on the part of those who have brought patients to the Asylum from distant parts of the state, has not unfrequently subjected them to considerable inconvenience and delay, and proved a source of perplexity and trouble to the managers.

The failure, in numerous instances, to furnish any history whatever of the cases presented, we deem worthy of notice on this occasion. It is a subject of so much interest and importance that we have thought it justly demanded the attention of the Legislature. A proportion of more than nine in ten of the patients admitted are paupers, and it too often happens that they are not only poor, but without friends or relations who take sufficient interest in their fate to procure such information as might be had in relation their cases. The importance of this subject need scarcely be urged. The Commissioners of the Asylum, duly impressed with it, at an early period after the opening of the institution, caused public notice to be given, in which they earnestly recommended to those who might conduct patients to the institution, to bring with them in writing, prepared if practicable by a physician, reports of the cases embracing in a particular manner the following points, viz. The age and habits of the patient—the length of time the disease had existed, with its supposed causes, and whether or not it had been hereditary in the family—whether the patient had been under medical treatment, and if so, how long, of what description and with what effect.

It will at once be seen that the foregoing interrogatories embrace the most essential points. We would however suggest the propriety of their extension to the following enquiries, viz. Whether the mental disorder was preceded by any known disease of the body? and if it was, the character of such dis-

case? Whether the patient has ever had convulsions of any kind? Have they any entirely lucid intervals? Are they subject to paroxysms of increased mental disorder? if so, do these occur with any regularity? are they preceded by any thing remarkable in the state of the patient's general health, or do any particular symptoms indicate their approach? Is there any particular subject on which the patient's mind is more insane than another? Is he or she harmless at all times? If not, state the particular mischievous or vicious propensities? In regard to the habits of the patient, those relating to exercise, diet, sleep and the use of ardent spirits, are most essential. In addition to the general statement, as to the hereditary character of the disease, it would be proper to state the result, if known, of the case or cases that may have occurred in the family. It would be interesting and proper also to be informed of the place of nativity and the occupation of the individual. Whether he or she has been married, and whether they have had offspring; and provided any communication from the Asylum to the friends of the patient should become necessary, where, and to whom it should be directed.

The information furnished under such a set of interrogatories would be highly interesting and useful, and would greatly aid and relieve the minds of the medical attendants of the institution: for whatever the experience or ability of a physician may be, unless he is furnished with the previous history of the disease he is about to treat, it will rarely happen that he can act altogether free from hesitancy and embarrassment. By the information thus furnished, in conjunction with a personal examination of the case, the medical attendants would be prepared at once to afford a fair and rational judgment as to the probable event of the case; to determine on the propriety, or not, of a course of medical treatment, and if medicine should be indicated, what particular agents, untried or partially used, would be most likely to effect the desired object; as also what course of discipline or of moral treatment would be most proper; and lastly to furnish, if desirable a full, fair and interesting history of the case.

In order to facilitate and secure the attainment of such information, we would respectfully suggest that the Legislature should amend the law in relation to lunatics, by making it the duty of the clerk or of the attorney for the Commonweath in every case, except of idiocy, to furnish the jury with the proper interrogatives in writing.

In addition to the regular attention bestowed on the Asylum by the commissioners, the Legislature have not failed, upon the annual return of their session to depute a joint committee of their body, to repair to the institution and thoroughly inspect its condition. This supervision is not without its salutary influence and is nothing more than the importance of the subject demands.

On a comparison of the statistical annals of this institution with those of others in the United States and of some in Europe and Great Britain, the contrast in most instances, is apparently much against us, except on the score of mortality in the foreign asylums. While in the institutions of a similar character in the United States, as we have recently had the satisfaction of observing, from an interesting paper on the subject by Dr. Beck of New-York, the proportion of recoveries varies from about 33 to 51 in every 100, and the number of deaths from 1 in 6¼ to 1 in 24¼, it will be seen from the statement which we previously exhibited, that the number of recoveries in the Kentucky Asylum, excluding idiots, is only in the proportion of about 29 in 100 and the deaths about 1 in 4¼. The rate of mortality in a majority of the foreign institutions, so far as our observation has extended, even exceeds ours, but the recoveries, in at least an equal majority, are more numerous. Our statement however presents at once a satisfactory explanation of our apparently unfavourable attitude on the score of recoveries, as it will be seen that out of 184 cases of insanity, which is the entire number admitted, excluding idiots, 152 may be regarded as chronic cases; and it is a fact that these were, for the most part of so hopeless and desperate a character that no means of restoration could rationally be expected to have any influence. The primary object of the Legislature in the establishment of the asylum being to

provide for the care and safe keeping of the insane paupers of the state, it might reasonably be expected that in its commencement, at least, a large proportion of the cases presented would be of the most hopeless character. The great mass of the community were entirely without information in relation to institutions of this kind, and the vague ideas which they gathered or entertained in regard to it, were of the most repulsive and shocking cast. They had heard of mad-houses and Bedlams in which the miserable victim of insanity was treated more as a ferocious wild beast than as an unfortunate human being, where he was subjected to chains, to the dungeon, to starvation and stripes; and it is by no means strange that on the first blush of this subject it should have presented itself to their minds with the most revolting aspect. Such indeed was the fact, and hence very few were brought to the institution except those of the very worst class of patients. Many, we have no doubt, who were entitled to gratuitous maintenance by the state, and had shared its bounty under the former system, but who had friends interested for their welfare, were withheld on account of these false impressions. Even up to the present period, notwithstanding the diffusion of more correct opinions among the citizens of the state, in relation to the management of the institution, a very large proportion of the cases that occur are chronic and hopeless before they are presented, and nothing more is anticipated than the safe keeping and comfortable maintenance of the individual.

As respects the degree of mortality in this institution, that too is to be accounted for, to a considerable extent at least, by a reference to the wretched character and condition of a large majority of the cases occurring. A more particular account of these would have presented a number very infirm from advanced life, and others labouring under great bodily infirmity and decrepitude from other causes. But although we are well assured that the foregoing considerations are sufficient fairly and rationally to account for an unusual degree of mortality in this institution, we are by no means satisfied that these alone are adequate. A disease, sometimes in the form of dysentery but

much more frequently of severe diarrhœa, has prevailed in the institution to an unusual extent ever since our attention has been given to it, and has terminated the existence of a large majority of those who have died. All the ordinary remedies for these diseases have been resorted to, but with failure of success in a very unusual degree. The disease has occurred for the most part in the worst class of insane patients, such as were fatuitous or idiotic in the extreme, and who were entirely disinclined to exercise or locomotion in any way. Our opinion has been that it was induced mainly by a want of regular and sufficient exercise, which, with this class of patients, is not easily obviated; vicissitudes of temperature have been, if we mistake not, the most directly exciting cause of the disease, as it has prevailed most fatally in the spring and fall seasons, particularly in the former, when such changes are most frequent and considerable.

Stone coal, as fuel, has recently been brought into use to some extent in this institution. It should be substituted for wood wherever it is practicable to do so, from several considerations. The first is its greater safety in avoiding accidents by fire, which is one of the first magnitude in every institution of this character, but especially in ours, the improvements of which, are so extensive and valuable, and in which, in case of an accident of this kind, the difficulty of placing the unfortunate inhabitants without the reach of danger, would be so great as to hazard many lives. Another consideration is, the greater regularity and duration of heat communicated from coal; which is likewise of importance in establishments of this kind, especially during the night, from the fact which we have noticed, that many of these wretched beings are incapable of attending to their own comfort, and after their retirement to rest, during the night, even in the coldest weather, will often abandon their beds and bedding, and stand or seat themselves on the floor, without a blanket for their protection; and we are satisfied that this does not arise from insensibility to cold, as has been imagined by some, because we have observed the same individuals who would act thus,

when in their day rooms, in cold weather, almost invariably hovering closely about the stove or the fire-place.

In this institution both moral and medical means are esteemed important in the treatment of the insane. By the exercise of uniform kindness and attention towards all who are committed to its care, they are induced to feel, as far as they are capable of being influenced by such conduct, that they are in the hands of those whose sole object is to do them service, and to contribute to their comfort and happiness. Their confidence is secured, if possible, by every appropriate expression of sympathy and commiseration. They are never imposed on by false representations or assurances; that if happily their confidence is attained, it may be steadfast. When required to submit to restraint in liberty or diet, or to the use of medicine, it is impressed on them that this is rendered necessary by some disorder of their health, on which account they have been placed in their present situation, where regular and proper medical attention could be given them; and that so soon as they are restored to good health they will be directly returned to their family and friends; that the medical attendants, and all around them, feel deeply interested for their recovery and desirous to render them every service in their power, and that they will be indulged in every thing that the state of their health will justify. When farther restraint, coercion or discipline is demanded, it is resorted to without the manifestation of passion or ill temper. Their most violent acts are met with moderation, firmness and composure, that they may be induced to feel that the power exerted over them, is not the offspring of cruelty or tyranny, but from necessity, and out of regard to their own welfare as well as of those around them. No harsh language or indecent expression is indulged towards them, on any occasion, and they are taught too, by reasonable checks and discipline, where it becomes necessary, that such will not be tolerated in them. In short, we may justly say that the moral treatment pursued is in all respects closely allied to that of well regulated parental government, except that the rod is spared, and in lieu thereof, the cold bath is used, either to the whole body or partially, by the affusion on the face and head, together

with some temporary privation of liberty.—In violent paroxysms of madness, leather shackles to the arms and feet, and the confinement of the patient on his bed, or in the tranquilizer* are found necessary. The strait jacket has also been required in some rare instances; but in the lapse of several years there has occurred but a single instance, within our recollection, in which a chain has been used, and that in the case of an individual who had acquired such extraordinary power and dexterity in effecting his escape from the Asylum, that none of the ordinary means of restraint were found adequate to prevent it. It is a rule in all cases that the restraint imposed shall be limited to the most moderate degree that the condition of the patient will justify.

We should have been more than gratified in being enabled to present the profession with some interesting and useful improvements in the treatment of this deplorable malady. But we are compelled to acknowledge that nothing has presented itself in our estimation deserving to be thus noticed. We will only observe that our pathological views on the subject of insanity, lead us to look attentively in all cases into the condition of the corporeal system for the morbid point from which the mischief has sprung, and to which remedial or restorative agents ought to be directed. We believe that if the physical structure and functions are perfect and healthy in their operations, the mental developements will be correspondingly clear and harmonious. It will consequently result that the medical treatment pursued is confined to no single class of remedies, but that it is varied and adapted to meet the particular morbid symptoms of each individual case, and accordingly as these are acute and violent, or mild and chronic, so are the remedies active and energetic, or only moderate and alterative. It may be remarked, in general terms, that purgatives, variously combined, and tart. emetic, chiefly in broken doses, have constituted the remedies most extensively used. Blood-letting, both local and general, and counter-irritation in various forms are also practised. As regards the last mentioned

* A kind of easy chair in which refractory subjects are securely confined.

class of remedies, we may notice that the ungt. tart. antim. has been repeatedly, extensively and perseveringly used, without any perceptible advantage.

Post mortem examinations are generally made. They have been directed more particularly to the brain, and such other organs as had presented symptoms of disease. In regard to such as have fallen under our notice we will only remark that in a majority of cases the brain itself, or its immediate appendages have been found in a state of disease.

The domestic concerns of the institution generally are neatly and well managed. Much labour and attention are bestowed in preserving the house and grounds clean, and in comfortable order. The keeper and matron, Mr. and Mrs. King, have received much just applause for the faithful manner in which they have executed this part of their duties, as well as for their kindness and indefatigable attention to the unfortunate persons committed to their charge.

On the first establishment of the institution, James C. Cross, M. D. was appointed resident or house physician. Andrew Stainton, superintendent, and Mrs. Stainton, matron. The medical faculty of Transylvania University, in a spirit of humanity and liberality which reflects much honor on them, tendered their services gratuitously as consulting physicians. Dr. Cross held his appointment but for a short time and resigned, when Wm. L. Thompson M. D. was appointed to supply the vacancy. He also resigned after a residence of a few months. At the close of the first year after the opening of the institution, a change also took place in the offices of superintendant and matron. Mr. Andrew M'Calla, an old and much respected citizen of this place, and at that time one of the commissioners of the Asylum, was appointed to the office of superintendent, fulfilling the duties also of apothecary and clerk. At the same period Mr. John King was employed as an assistant, and Mrs. King, matron. At the commencement of the year 1826, the commissioners did me the honour to confer on me the appointment of attending physician, making it my duty to visit the institution every day. On the 1st of April, 1829, the office of superintendant, apothecary,

and clerk were vacated, and shortly after J. C. Jordan, M. D. was appointed resident or house physician, fulfilling the duties also of apothecary and clerk. On the 1st of October last, my services to the institution were discontinued, under a resolution of the board of commissioners, expressing, that, having found it necessary to appoint a resident physician, the services of a daily attending physician were not deemed requisite. The board were pleased in the same resolution to tender an expression of their approbation of my services by a vote of thanks. The Medical faculty have continued the regularly consulting physicians and have on all occasions, when called on, promptly and cheerfully rendered their services. They are authorized by a resolution of the board of commissioners to issue tickets of admission to the Asylum, to the members of the medical class without charge. The commissioners at this time are, John W. Hunt, Chairman, Richard Higgins, John Brand, Richard Ashton and John H. Morton.

Although this institution was established by the state of Kentucky, with the view particularly of providing for the proper care and maintenance of its insane paupers, the commissioners have felt authorized, so long as there were surplus means of accommodation, to extend its benefits to others, on the payment of a very moderate compensation, about three dollars per week, for boarding, washing and lodging, and without charge for medicines or medical services. Under this prudent and humane exercise of discretion on the part of the commissioners, not only citizens of our own state, but in several instances those of other states have been allowed to share the advantages of the institution.

The Asylum is yet in its infancy, but with the lights of experience afforded by other institutions, together with the munificent and humane spirit of our legislature, and the extraordinary attention of the managers, we flatter ourselves that it has rapidly advanced to an equality with the best regulated institutions of this kind. Time and means are yet wanting to enable the commissioners to devise and execute all that can be effected towards the most useful and elegant improvement of the grounds.

It cannot be doubted that much may be done in these respects by the properly directed labour of the inmates of the house, but an important preparatory step is a suitable enclosure.

ART. IV. *An Essay on the Absorbent System and its Functions,* By JAMES CONQUEST CROSS, M. D.

PREFACE.

TO periodical essays we know it is not customary, and perhaps hardly excuseable, to prefix formal prefatory remarks, yet, we hope the necessity of this will appear so obvious, as to render apology superfluous.

The difference between the obvious properties of the vessels of the absorbent system, and those of the capillary arteries, has been, I hope, satisfactorily demonstrated. Authors have introduced much confusion into their writings by not recognising this distinction. By assigning to them properties almost identical, they have confounded the capillary arteries, exhalents, secernents, and absorbents. It has been shewn that what have been denominated exhalents and absorbents are the same in their properties, and their functions; but the properties of these vessels have been shewn to be very unlike those of the capillary arteries. The neglect of this distinction has rendered our ideas of exhalation and absorption vague, inconclusive and confused, and has contributed much to revive the exploded Boerhaavean doctrine of venous absorption.

If we turn our attention for a moment to the manner in which the arteries terminate, according to Haller and all the authorities of note since his time, we cannot fail to be struck forcibly with the remarkable want of precision prevalent on this subject. Thus, we are told by Haller and his successors, and it appears this information is derived from injections, observations with the microscope, and the obscure and uncertain lights of analogy, that when an artery is pursued to its last divisions, it terminates di-

rectly without any interposing tissue, either in a vein, an excretory duct, an exhalent, a lymphatic, or very minute arteries that contain a colourless fluid. From similar data, the veins are supposed to originate in the same manner that the arteries terminate. If we could by any exertion of the imagination fancy this statement true, we could not avoid the irresistible inference that the capillary systems of Bichat are a heterogeneous compound of exhalents, excretories, absorbents, capillary arteries and veins, more unintelligible than the witches cauldron in Macbeth.

A positive inconsistency is implied in the assertion, that the capillary blood-vessels are identical with, and also give rise to the exhalents, excretories and absorbents. If the former give rise, as they really do to the latter, it is preposterous to say, that they should be classed together. Indeed, the circulation in the capillary blood-vessels, and the functions of the exhalents and absorbents, have been so completely locked up in inscrutable mystery by the extremely crude, vague, mechanical and erroneous ideas to be found in books generally on the subject of the tissues by which they are performed, that the utmost that has been said of them in a systematic manner, is, that they are functions of the living economy, without being able to recognise any portion of fixed and unfluctuating science.

This we flatter ourselves cannot be urged with propriety against the view presented in the ensuing pages;—it cannot be charged with that entire destitution of precise classical arrangement, and clearness of thought so remarkable in those already before the profession. It may be false, and must, in common with all human productions, be necessarily defective—but there is no mystery about it—it cannot fail to be understood. Although, from the imperfect state of anatomical science, some points may not be supported by evidence sufficient to enforce general conviction, we are persuaded from what has already been discovered, that when the anatomist shall have succeeded in unfolding the intimate nature of the more delicate, concealed and recondite tissues of the body, their truth will be established by proof positively irrefragable. Should we, however, indulge

a belief unwarranted by the disclosures of the future anatomist, we still must labour under the conviction, that the track here pointed out will lead to the most important developements, and perhaps direct the unsteady footsteps of the enquirer through the bewildering mazes of anatomical research. For, although the structure and functions of the grosser organs of the body may be pretty thoroughly understood, it must be confessed by the candid and impartial cultivator of medical science, that both physiology and pathology are yet in their infancy; that much obscurity still hovers over many of the functions of life; that numerous abnormal changes are perfectly unintelligible; that with our present stock of knowledge they are entirely inexplicable, and that if they are ever understood, it must be through the agency of a more intimate acquaintance with the finer textures of the body.

Before concluding these prefatory remarks, let me not disingenuously disguise the fact, that in the composition of the ensuing essay, I have been under great obligations to M. Alard. But while I freely make this disclosure, I obstinately and decidedly refuse to admit, notwithstanding there may be considerable likeness between his opinions and those advanced by myself, that they were suggested by a perusal of his work. My opinions have been familiar to my professional acquaintances for years:—so early as 1823 they were avowed in the Lexington Medical Society. In the years 1824—25—26—27, I read several essays before the Society, on generation, fever, nervous action &c. in all of which the opinions contended for in the essay now submitted to the public, were distinctly avowed. In the debates during those years in which I freely participated, the same opinions were my constant theme, so much so, that the absorbent system was proverbially called my *hobby*. In the year 1825, I delivered a course of lectures in Lexington, in which my opinions were in a detailed and circumstantial manner developed. The first intimation I received that I held opinions entertained by M. Alard, was from a French periodical:—I immediately wrote to Mr. William A. Leavy, who was at that time in Philadelphia, for the work, which he purchased of Messrs. Carey and

Lea, and it reached me for the first time early in the autumn of 1826. It was not till the summer of 1824, that I commenced studying the French Language, so that it is perfectly impossible for me to have derived from M. Alard, what I urged upon the subject previous to that period. If, however, the principles contended for shall be found true;—if I shall be considered as merely the organ of M. Alard, in communicating to my countrymen more enlarged and enlightened views of the subjects of which I have treated;—if the interests of medical science shall be subserved, my ambition will be satisfied and the public will find me as much disposed to do M. Alard ample justice, as his warmest friend in the professional community. His work has afforded me numerous facilities, and I feel proud that I am not ashamed to avow it. *December 6th*, 1829.

Having in the 38th No. of the American Medical Recorder, espoused the doctrine of the circulation of the blood, as taught by the illustrious Harvey, and solicitous for its establishment, we have determined to embody all the knowledge we have been able to collect, either as the result of bibliothecal research, personal observation or reflection, into a condensed form, which, as we conceive, will serve as a very natural introduction to the study of the absorbent system and its functions.

The discovery of the circulation of the blood, we consider the unalienable and indisputable property of Harvey, notwithstanding the too zealous friends of Cisalpin, and those of Servetus (an obscure Spaniard) have attempted to rob him of that immortal glory, which has made his name familiar with posterity, and covered it with imperishable renown.

The triglochine, mitral, sigmoid valves, and those also, frequently found in the veins, are so arranged, says Harvey, as at once to suggest the idea, that the blood passes from the veins into the auricles; from thence into the ventricles and then into the arteries. They also prevent, he contends, the retrograde motion of the blood. In common with Cisalpin, he observed, that the veins swell below a ligature, while a reverse state obtains with

regard to the arteries. The continuity of the arteries and veins, is an anatomical fact of which he was not ignorant.

The division of the circulation by Harvey, we consider much superior to that more prevalent and popular one of Bichat, in precision, correctness and comprehensiveness. It is divided by the former, into great and small, or general and pulmonary circulations; by the latter, into arterial or red blood, and venous or black blood, and is objectionable on account of its being general, when it should be particular,—loose and unscientific when it should be logically precise. Thus, the pulmonary artery instead of circulating red blood, circulates that which is venous, and the pulmonary veins arterial, instead of black blood. His idea of the arterial circulation presenting a perfect circle is gratuitous and unfounded, for it can be viewed in no other light than as a part of that figure. To the venous circulation the same remark is applicable.

In addition to the facts and arguments recorded in the journal alluded to, the following considerations will contribute to enable us to ascertain how far the vis a tergo of the heart extends, and we humbly hope, will serve in a great measure to rescue from neglect, that doctrine which has been with unphilosophic precipitancy, consigned to the shades of oblivion.

The vital physiologist has had the accumulated experience of too long a series of ages, to be led away by the fanciful visions of that host of calculating philosophers, that would determine with mathematical precision, the distance the power of the heart is capable of propelling the blood. In vain may Hales, Keil and Borelli, calculate;—their exact computations may amuse the credulity of the partisans of geometry, but can never satisfy the inquirer into the laws of the vital functions. The action of the heart, presents a phenomenon destitute of a parallel in all the operations of inanimate nature; it is perfectly *sui generis* in its character, and surpasses in beauty and excellence of adaptation, all the sublimest productions of human genius. The heart, bears in its structure the indelible impress of a hand, omnipotent in the wide and illimitable sphere of its operations. Vain and vexatious then, must be the poetical soarings of that imagination,

that would chain down to the rules of technical precision in sciences, more exact than that of life, the mysterious workings of an organ, which exhibits in its structure and action, the manifest and inscrutable agency of super-human power. In some other manner then, more consistent with the dignity and grandeur of the subject, must we contemplate the circulation of the blood, if we would reach the summit of physiological excellence, on which truth has raised her distant and almost inaccessible temple. Chemistry with her multifarious train of affinities, of compositions and decompositions, and mechanics with her numerous retinue of exact computations, must be exiled from the study of man's living economy, and that extended chart, in which all his normal and abnormal operations are minutely and accurately depicted, must be studied with the deep and wily penetration of a trained philosopher. Nature must be viewed in her own works, and not in the productions of human hands;—conclusions must be deduced from facts and phenomena, as they actually exist, and not from the vague, seductive and inconclusive analogies of particular sciences, which have no natural alliance with the science of life.

Without attempting to calculate the precise power of the heart, if it can be established that its left side exercises a manifest and efficient influence in the veins, independently of any agency derived from the right ventricle, every theory, but that of Harvey, must be expunged from every code of sound physiology. In accomplishing this, facts incontestable in their nature shall be our only guide, and in this spirit we proceed to lay before the reader, the evidence which may be considered supplemental to that contained in the essay alluded to.

It is stated by the celebrated John Bell, while speaking of the effects of irritating a frog's foot, that we may "see the influence of the heart's action upon the blood in these minute veins, for at each pulsation of the heart the red globules are sent forward, being stationary, or recoiling during the diastole." It is the deliberate opinion of the illustrious Dr. Armstrong, "That the forcing power of the heart extends from the arterial to the venous system appears to me quite obvious; for whenever the heart's

action is increased, then the current of blood from a punctured vein is increased, and when the action has been very excessive, I have known the blood come out in jets." We are informed by Dr. Fodere that he has frequently seen delirium announce the fatal termination in acute peripneumony; the face being bloated, the cheeks of a violet colour, and the jugular veins pulsating. In varicose states of the veins, when perhaps, the communications between the arteries and veins are more free than usual, the veins, it is asserted upon high authority, pulsate synchronously with the systole of the heart.

In the *Archives Generales* for 1823, Dr. Bouillard records the case of a woman aged 68, who laboured under strong pulsation in the region of the heart, and pulsation also of the jugular veins. She died, and when a necroscopic examination was made, the left ventricle was found dilated, and its parietes in a hypertrophoid condition, being about an inch in thickness. In the Journal Complement, we find the case of a soldier who was attacked by hemiplegia, related by Dr. Breyer. The disease continued several days, when an universal pulsation in the veins was remarked. The venous pulsations were perfectly synchronous with those of the heart and arteries. The whole surface appeared to rise and fall in correspondence with these pulsations, which were visible in the eyes and tongue. This phenomenon continued five days, and then disappeared. On the same evening the patient became comatose; on the 6th day from this time the venous pulsation again returned, but the patient still continued insensible. The pulsation subsided on the second day from the latter period, and in two days more he died. Dissection threw no light upon the cause of the venous pulsation. In the Dublin Hospital Reports &c. for 1827, Doctor Davis gives us the history of a case that occurred to him. The subject of it was a female æt. six. Four months previous to the time when the case came under the care of Dr. Davis, she had been afflicted with pertussis, and at the time he was called to her, she laboured under acute hydrocephalus. What surprised him much, he remarks, was a pulsation in the veins, distinct and well marked, synchronous with that of the arteries. In the veins of the ex-

tremities, the pulsation was perceptible to the eye, even at the distance of two yards. The veins were rather larger than is customary at her age, and pressure upon any of them stopped the pulsation between the part compressed and the heart, so that it obviously could not be caused by regurgitation from the auricle. The pulsation in the part of the vein towards the extremity, was rendered much stronger, and more distinct, provided the return to the auricle was not completely obstructed, but if the compression was so strong as entirely to obliterate the calibre of the vein, that part of it which became tense and distended with blood, so far as the next valve, after a few seconds, lost the pulsation altogether. Together with some traces of pathological action found on dissection in other parts, the heart was hypertrophoid, and firmer than natural. The arteries were injected, but no preternatural communication between the arteries and veins was discovered, consequently, we are obliged to derive the venous pulsation directly from the left side of the heart. In the month of October 1821, a child of this city, (Philadelphia) says Dr. Rush, aged about thirteen years, was attacked with bilious fever. He was ill for a long time, but eventually his disease left him in a very debilitated state. He was remarkably emaciated—so much so, that curiosity one day induced him to examine his skeleton-like arms, when upon a closer view, he saw the superficial veins upon the back of his hands distinctly pulsate, synchronous with the beat of the heart and arteries. It was less evident as the child advanced in convalescence, and when he recovered his strength and flesh, the pulsation of the veins entirely ceased.

I should have remarked above, that Haller informs us, that venous pulsation is frequently felt in the jugulars, in the abdominal veins and even in the external iliacs, but he absurdly attributes it to regurgitation. To see the blood flowing per saltum from a vein is not uncommon, and has doubtless been observed, by every practitioner, but it is rare to see venous pulsation very manifest.

"It will hardly be argued," says Dr. James Johnson, while commenting upon the case of Dr. Brayer, "that in this the influ-

ence of the heart's action ceased when the blood arrived at the capillary vessels. The fact above narrated without any thing in view, is a strong support to the doctrine of the heart being the great moving power of the circulation throughout the whole circle from the left ventricle to the right auricle." Speaking of the case of Dr. Davis, he says, "it proves beyond a doubt that the force of the heart may and does act all round the course of the circulation. If the heart be capable of producing a pulse in the veins, under extraordinary circumstances, how can we doubt that it acts in propelling the blood more or less in the veins, under ordinary circumstances."

Indeed, it appears to us, that the most rash and zealous espouser of Bichat's capillary systems, cannot after a sober and deliberate examination of the evidence furnished by the above cases, come to the conclusion, that the heart's action is limited to the commencement of the capillaries. In fact, proofs so unequivocal and striking in their character, have rarely, if ever, been marshalled in defence or support of any proposition, contained in the codes of physiology, with which we are acquainted. No other explanation we conceive can be given of the above phenomenon than that furnished by the doctrine of Harvey, and every hypothesis, that fails to account satisfactorily for venous pulsation, must be essentially erroneous. But an inquiry into the cause of venous pulsation can never lead us into error;—no doubt hovers around the subject;—no cloud throws its shadowy and unsubstantial form between the phenomenon under investigation and the person of the observer;—no metaphysical sublimation or awakening of intellectual sensibilities is necessary in order to enable the physiologist to have a clear, distinct and convincing view of the singular operation going forward. When we feel the radial artery pulsate under the finger in perfect harmony with the action of the heart, all are willing to admit that it is legitimate to infer, that the latter is the cause of the former; —if then, we should place the finger upon the external jugular, the vena saphena or any other large superficial vein, and observe the same phenomenon, and the same harmony of action, between the heart and the vein, who will say that it is a non sequi-

tur, when we infer that it is the action of the former which produces pulsation in the latter.

Convinced that the evidence above adduced, in favour of the opinion that the heart is the exclusive motive power of the blood, is entirely unanswerable, we proceed to invalidate, if possible, the most conspicuous objections that have been urged against it.

The opinion of Harvey, has been arraigned on account of its apparent incompatibility with the emptiness so characteristic of the arteries in the dead subject. This inconsistency, however, is only apparent, for the strength of the heart, diminishing in a ratio corresponding precisely with the direct approximation of dissolution, the quantity of blood driven into the systemic arteries, must necessarily be reduced in the same proportion. This reduction of quantity sent into the arteries, must be accompanied by a proportionate increase in the quantity of blood in the veins. Under ordinary circumstances therefore, there will be an emptiness of the arteries and a fullness of the veins.

An objection is urged by Dr. Cartwright, founded upon the emptiness of the crural artery and the plenitude of the crural vein, as exhibited in the experiments, recorded by me, in the 38th number of the American Medical Recorder. In his very ingenious Essay on Venous Absorption, he thinks that the experiments performed by Magendie, and myself, prove the reverse of what was intended. He says, in allusion to these experiments, "But this, with the other experiments of Magendie and Cross, afford positive evidence against the doctrine of the vis a tergo, and establish, I think, beyond a doubt, that the motion of the venous blood depends on atmospheric pressure. For if the motion of the venous blood was produced by the vis a tergo of the heart, *the blood could not be forced up into the veins so as to make a plenum there after the arteries were firmly tied.*" No other result, contemplating the experiments *a priori*, in reference to the sole agency of the heart, could have been rationally anticipated. Could it be expected, that the blood moving under the energetic influence of the heart, could have lost its impulse in a moment, and have become suddenly motionless? Would not this

impulse of itself, after all communication with the heart was cut off, be sufficient to expel the blood from the artery? And if so, —what could be expected but a plenum in the vein? Moreover, if the motion imparted to the blood in the artery, at the time it was tied, was not sufficient to carry it into the vein, do we not know, that it was at least sufficient, to remove it some distance from the ligature, that when the artery ceased to be a plenum its contractility of tissue, which is in all circumstances acting, would now act, with increased energy, and by an exertion almost passive, if I may use the expression, force it into the vein. This corresponds precisely with the experiment: for supposing that the motion, imparted to the blood, before the ligature was applied, emptied the artery and filled the vein, we could not expect, that it would cease to issue from the latter immediately and suddenly; and this is what actually takes place, for Magendie says "le jet de sang veineux ne s'arretera pas pour cela, il continuera quelques instans; mais il ira en diminuant, et l'ecoulement finira par s'arreter, quoique la veine soit pleine dans toute sa longueur."

In conclusion, we would remark before leaving Dr. Cartwright, that if according to his hypothesis the venous circulation depends upon atmospheric pressure, and it was this agent and not the heart which in the experiments above alluded to made the crural vein a plenum, it should have completely emptied it instead of suffering it to remain full.

A want of correspondence between the throbbing or pulsatile sensation sometimes experienced in a part labouring under inflammation, and the contraction of the heart, has been considered as an insuperable objection to the doctrine espoused by us. This objection, however, is deduced from data, that have no foundation in truth, and in the language of logicians is a non sequitur. Those who have urged it, attribute inflammatory pulsation to contraction of the arteries. But this is not true: for while it has not been proved, that the arteries are contractile, this attribute belongs according to the testimony of the highest authorities, to the vessels of the absorbent system. It is contended by the most enlightened pathologists, that inflammation

is seated in the capillary arteries, and that if the propulsive power of the heart, circulates the blood without any assistance from the vessels themselves, the pulsatile pain felt in a phlegmon should be synchronous with the systolic action of the heart. If the location of inflammation were correct, this inference would be fair, but as we have ventured to deviate from the reigning pathology on the subject of inflammation, and in conformity to some ancient authorities, to transfer its seat to the vessels of the absorbent system, it is, according to our conceptions, illegitimate. The absorbent vessels, as shall be evinced in the sequel, are different in every prominent characteristic from the capillary arteries, and are, alone of all the vascular tissues of the body, capable of giving a satisfactory solution of the multifarious phenomena of inflammation. If we are thus authorized, in changing the seat of this disease, it must appear obvious to the reader, that there is no absolute reason, why the pulsation of a phlegmonous tumour, should correspond with the contraction of the heart. Although this is not a suitable occasion, to enter into a detailed statement of the reasons which have induced us to depart from the prevalent pathology of inflammation, the reader will pardon me for remarking, in order to render our argument plausible, that inflammation is almost, if not quite universally, seated in colourless tissues;—that these tissues abound with absorbents, to such an extent, as to be almost exclusively made up of them;—that these tissues while they retain their healthy aspect, are normal, and when they lose it by the introduction of blood into them, they take on that kind of abnormal action which constitutes inflammation. Now, we would ask, who is willing to suppose, that if the undemonstrably minute vessels of these tissues were capillary arteries, that the introduction of blood into them could give rise to disease? If they are real arteries, as well might we suppose, that the introduction of bile into the gall-bladder, of gastric juice into the stomach or of urine into the urinary bladder would cause disease, as that the introduction of blood into an artery would excite inflammation. Indeed, the prevalent opinion is so very preposterous—is so entirely unsustained by the phenomena of inflammation, that we can-

not refrain from an expression of our astonishment, that a single enlightened pathologist ever became a proselyte to it.

Were we, however, to subscribe to the reigning opinion of the seat of inflammation, we cannot see how this would facilitate the explanation of the pulsatile pain in a phlegmonous tumour; for the want of correspondence in the action of the heart, and that of the swelling, must proceed from some cause foreign to the former and peculiar to the latter. This, perhaps we will be told, may be found in the muscularity of the arteries. The errors however, into which that doctrine would lead, which teaches, that the pulse is independent of the heart, do not appear to have been sufficiently considered. For, if the arteries have an action sui generis, the pulse can be no longer viewed as a sure index of the physiological and pathological states of the heart.

The muscularity of the arteries, however, has not been proved. Indeed, those who inefficiently contend for it, do not appear to be aware of the absurdity, in which they would involve themselves. For, if there is arterial muscular contractility, the vessels which circulate red blood, must necessarily possess, in common with the splanchnic muscles, a degree of irritability, commensurate at least, with that belonging to the latter. If this were true, we should see the arteries contracting after the heart had ceased to beat;—we should see, in contravention to an unequivocal truth, the arteries pulsating during syncope. It has been urged, that the alternate contraction and dilatation of the arteries which pre-supposes muscularity, would give rise to a vacuum, which would remove the blood from the arteries on the principle of exhaustion. Though this doubtless would contribute considerably to accelerate the motion of the blood coming from the heart, we are unable to conceive, how this vacuum is to influence the circulation in the veins, towards the right auricle. We can see very well, how it would be retarded: for as it is not contended that there are other vacua in the veins, the blood would be unavoidably arrested in its progress, and thus, what would be gained by it in the arteries, would be lost, for the want of it, in the veins.

These considerations induce us to believe, that some more satisfactory elucidation must be sought, for the phenomenon of phlegmasial pulsation, than that furnished, by the supposed muscularity of the arteries. The view we have taken of the seat of inflammation will unloose without difficulty the Gordian knot. When the absorbents are inflamed, we are told by Mr. Travers, that a heavy pulsatile pain is one of the symptoms by which it is characterized. This is a disease, according to pathologists, of a peculiar nature;—it is evidently beyond the reach of the torrent of the circulation and the action of the heart. Consequently, this "heavy pulsatile pain," can be attributed, neither to the action of the heart, nor the contractility of the arteries. Muscularity and contractility are conspicuous attributes of the vessels of the absorbent system; thus, the pulsatile pain is explained. Inflammation viewed in this light, it does not surprise us, that a phlegmonous tumour should pulsate, nor, that it should not correspond with the systolic action of the heart.

The late Professor, N. Smith, objects to this exclusive doctrine of the circulation, for the following reason: he remarks, "In every case of local inflammation, before the action of the heart and arteries is in the least changed, there is an accumulation of blood in the capillaries of the part inflamed. This could not, indeed, take place, if the blood was not furnished to them by the heart and arteries; but, in such cases, the action of the heart is not directed to the inflamed part more than it is to every other part, nor more to that particular part than it was before the inflammation took place. It follows, therefore, that the change in the part diseased, producing an increased quantity of blood and an increased evolution of heat, must depend on some change in the action of the vessels of the part, and not at all on the *vis a tergo* of the blood impelled by the heart. It is true, that Harvey's doctrine of the circulation, when viewed in connection with the prevailing opinion of inflammation, inculcates the idea, that the determination of blood in morbid quantity to a part, depends upon, the direct propulsive agency of the heart, but, when we transfer the seat of inflammation, from the capillary arteries, to the vessels of the absorbent system, the reason why the symptoms of local in-

flammation are developed, before the action of the heart is found to be disturbed, is made perfectly obvious. The blood, which constitutes the accumulation, is removed from the torrent of the circulation, and beyond the reach of the vis a tergo of the heart; it is evident, therefore, that any change which takes place in the heart or any other part, must be secondary, and the effect of the morbid condition of the mass of absorbents labouring under inflammation. It may be said, that the disturbance in the functions of the heart succeed too soon the local phenomena, for the former to depend upon the latter for its cause. To which it may be answered, that every inflammation must be originally necessarily local; for though, a general predisposition may exist, a general inflammation ab initio is a desideratum in pathology, and that, therefore, it arises independently of the heart. The rapidity with which the disturbance of the heart succeeds the local phenomena, is no argument against the view above expressed, for syncope, which is unquestionably a disease of the heart, is in some individuals, produced instantaneously, from tickling the dermoid organ. With this modification, we are prepared to unite with Professor Smith, in the consequence which he deduces; i.e. "*that the change in the part diseased*," &c.

The deservedly distinguished Professor Caldwell, in a triumphant refutation of the doctrines of Dr. Barry, remarks, that "the fact, that the capillaries perform on and with the blood, a variety of functions important in themselves and essential to the body, is itself satisfactory proof, that that fluid is not forcibly and rapidly driven through them by the influence of the heart; but that it circulates by virtue of their own action. Hurry it along by an extraneous power, and their functions performed on it, will be necessarily prevented." The phrase, *capillaries*, alludes doubtless to vessels, that circulate blood, and if so, two radically different functions, i. e. circulation and secretion, are assigned to the same tissue. This is at war with all that is known of the functions of life: difference of texture always gives rise to difference of function, and vice versa. It is true, that any morbific agent, that accelerates the circulation will impair the functions of absorption and secretion, but the author should recollect, for

it is a remark deducible from the principles he has been teaching for twenty years, that no cause capable of producing this effect, can act primarily upon the blood, and that, the increased velocity of the circulation, should be attributed to a previous disturbance in secretion or absorption.

We were not aware before, that the "blood moves through the capillaries much more slowly than it does through the arteries and veins." We have always been of opinion, that unless a perfect uniformity in the momentum of the circulation was observed, disease must ensue. It is a fact of frequent observation, that a kind of congestion of the capillaries arises, either from a morbidly vigorous or languid circulation in any part of the circulatory circle. Thus, when the circulation is too active from a hypertrophoid condition of the ventricular parietes of the heart, together with an enlargement of the cavities, the capillaries become congested. When also this hypertrophoid condition is associated with a contraction of the cavities, the circulation is so languid as scarcely to reach the arterial extremities. When, therefore, an excessive quantity of blood is sent to the arterial extremities, it accumulates in the capillaries, produces compression, and embarrasses the circulation. When, also, too small a quantity is sent, the fluid does not receive a sufficient impulse, for its full and entire circulation, from which a congestion of the capillaries unavoidably results.

From these considerations, and those contained in the number of the Recorder alluded to, we feel ourselves authorized to conclude, that the circulation is a function under the exclusive control of the heart. This, then, implies a disbelief in the existence of capillary systems, separate and distinct from the arteries and the veins. Contending, therefore, as the doctrine of Harvey does, for the continuity of the arteries and veins, it is perfectly impossible for us to sanction the ideas, that prevail on the subjects of absorption and exhalation.

Before we enter upon the consideration of the absorbents and their functions, it is important that we should say explicitly, what we wish to be understood by several phrases which we shall be under the necessity of frequently using.

in which a circumstantial account of them is contained, will convince the reader that this was rarely the case.

To illustrate the true nature of the elementary tissue of the body, we shall now take into separate consideration, several of the textures, and in the first place, we call the attention of the reader to the

Encephaloid Tissue.—Though such evidence, as will satisfy the senses, can never, perhaps, be adduced in favour of the existence of absorbents in the brain, we do not despair of being able to bring forward such proof, as will answer all practical purposes. There are no symptoms that denote with pathognomonic precision, the exact moment when effusion takes place in the ventricles of the brain, but that such is occasionally the fact, and is afterwards removed, the multiplied and multifarious results of clinical observation will not suffer us to doubt. The phenomena of growth and nutrition, are quite conclusive on the subject, for it is a fact that universally obtains, that where there are nutrient vessels, there are also, those that remove the old and effete matter. Besides the evidence derived from healthy nutrition, the clinical observation of M. Dance furnishes us with two cases of hypertrophy of the brain, which, in his opinion, establishes the existence of nutrient vessels in that organ. He considers this state of the brain "the effect of morbid nutrition: and if so, it is not difficult to conceive that the brain submitted to an uniform stimulus of nutrition, must become eventually more dense in a similar uniform ratio." More satisfactory evidence of the existence of absorbents in the encepalon cannot be required, than that furnished by a case of Dr. Heustis. In a case of Hernia Cerebri, the fungus disappeared entirely under the influence of pressure, and its place was occupied by a hole, in the substance of the brain as large as a hen's egg. "The unsightly cavity," he remarks, "left in the brain after the removal of the fungus by pressure, might excite the apprehensions of the inexperienced practitioner; but this is a circumstance of no moment; no unfavourable symptoms are occasioned by it; nature is fully competent to the work of regeneration, and in the brain this process

appears to progress with more rapidity than in any other part of the body."

Until the researches of Professor Lobstein of Strasburg, shed considerable light upon the minute anatomy of the brain, few, if any, believed that absorbents entered as an element into the encephaloid tissue. It is asserted by Mascagni, that there are absorbents in the brain; he says, he has seen them and has given us a plate representing them.

One of the most conspicuous properties of the absorbents is contractility, and if this property is manifested by the encephalic mass, as there is no other tissue which enters into its structure capable of exhibiting this phenomenon, we shall be under the necessity of ascribing it to the absorbents. The encephalon has a motion we are aware, which is derived from the action of the lungs and heart, but it has one also we conceive inherent in its organization. Thus, we are informed by Littre, that in a man who committed suicide by violently striking his head against a wall, the brain was found contracted and reduced in size. The same phenomenon was observed in the brain of a man, we are told by Dumas, who was killed by being thrown from a window. In some experiments performed by Lorry upon dogs, the same result was obtained. These animals were destroyed by being stricken on the head.

We cannot give a plausible solution of the determination of certain fluids in morbid quantity to the brain, unless we admit the existence of absorbent vessels in that organ. Thus Lapeyronie used wine in dressing a gangrenous abscess, the impression of which was transmitted to the brain, which enlarged it so enormously in a few days, as not only to destroy the patient, but absolutely to force the bones of the cranium asunder. Two other cases not unlike this, are recorded by Lecat; in one the abscess was in the brain, and the other in the spinal marrow.

With regard to the membranes of the brain, Dr. Horner remarks, that the deep seated absorbents of the head have been traced to the membranes but no farther. Between the tunica arachnoidea and pia mater they were observed by Ruysch, when inflated by air and were denominated by him vasa-pseudo-lym-

phatica. It is asserted by Lancisius, and Pacchioni that they detected absorbents in the pia mater.

If the following observation extracted from Professor Dudley's very able memoir on Injuries of the Head, does not evince their existence in the brain, it is so conclusive on the subject of the membranes, that to deny them there, would be nugatory and preposterous. "After the operation" (trephining in a case of Epilepsy) says the Professor, "on turning my attention to the dura mater, I was astonished to discover a copious secretion of fluid which separated that membrane from the surface of the brain more than an inch; while that organ in place of giving to the finger a sense of pulsation, felt as hard and as unyielding as a board. The patient being dressed in the usual way, was put to bed and kept quiet until the fifth day, when from the establishment of free suppuration, the bandages were removed. It was now ascertained on examination that the fluid beneath the dura mater was absorbed."

Nervous Tissue.—The nerves according to Bichat have a great quantity of cellular tissue, and this tissue we know abounds with absorbents. By Mr. Descot, we are told, that blood-vessels, very large relatively to the nervous fibres, are distributed to the neurilema of the nerves, and penetrate into their internal structure. Now, the distribution of blood-vessels to this tissue can be for no other purpose than to nourish it, and this function cannot be performed by them directly—there must be some intermediate agent, which agent is the arterial absorbo-exhalents of which the reader shall be satisfied as we proceed. That the nerves inflame, we are assured by Reil, who states that a man who died of typhus after enduring exquisite pain, had these organs deeply coloured with blood, and after the neurilema had been destroyed by nitric acid, it was found that the blood had penetrated the medullary substance itself. By Mr. Descot we are told, that when a nerve inflames it may be perceived in a few hours to swell, become of a dark red, and very vascular. M. Beclard admits that they may be inflamed, but that it is ever idiopathically, he thinks very problematical. Inflammation,

no matter in what organ developed, I believe to be generally if not universally situated in the cellular texture of the part.

Beclard, declares that suppuration, ulceration, and gangrene, may occur in the nerves. Not only these processes go on in the nerves according to Descot, but they also undergo cartilaginous, osseous, calcareous transformation and other morbid changes of structure. Can it be supposed, that these pathological states can obtain, independently of the agency of the absorbents?

It is asserted by Descot, that divided nerves re-unite in the same way with all other parts, and their reunion is speedy and exact in proportion to the accuracy with which their ends are retained in apposition. And the experiments of Fontana upon the eighth pair, and the intercostals, prove conclusively, that nerves regenerate in the same way as the absorbent vessels and cellular tissue. This is not at all improbable, since it is ascertained, that the brain itself may be regenerated.

It has been demonstrated by Home, that the nerves are endowed with the faculty of contracting when they are cut; a phenomenon which induces the supposition that there is great analogy between the nervous filaments and the transparent vessels of the absorbent system, a supposition strengthened by the fact, that according to Reil they swell, and that this swelling softens them, and augments their irritability, a property with which the small vessels of the nerves in common with the absorbents are amply endowed.

That the nerves are really tubular we are scarcely permitted to doubt. Thus Leuwenhoek examining through a microscope the optic nerves of a very large animal observed distinctly a multitude of vascular filaments full of transparent globules. Vieussens has seen through the transparent tunics of the nervous filaments a congestion in them of a crystalline fluid. We are assured by Malpighi that from the cut extremity of the large nerve of the arm he saw effused a considerable quantity of lymph that resembled the white of an egg. Lecat professes to have witnessed several cases of this kind. Though

this effusion presupposes a pathological state of the nerves, it effectually demonstrates the nerves to be tubular.

In the experiments of Reil, and Fontana, the tubular form of the nerves is rendered strikingly conspicuous, while in more recent times, by Bogros it is asserted, that he has demonstrated by minute injections that the nervous pulp is hollow in the centre of the nerves. His experiments have been repeated by other anatomists, and although they are confirmed by some, others have not been able to obtain similar results.

Cellular Tissue.—We do not select for investigation this organ because the existence of absorbents in it is disputed, but because it is better qualified than any other, to give a correct idea of the great extent of the absorbent-system. It is of paramount importance, both in a physiological and pathological point of view; for being circumscribed by little less than the whole extent of the animal body, and entering as an essential element into almost every structure, it is quite impossible for any change, either of a normal or abnormal character, to take place without involving it, either in a direct or indirect manner. Indeed, were every other capital ingredient of the human fabric to be removed, the deficiency in the opinion of some anatomists could scarcely be observed. The same elegant symmetry of form, fascinating beauty of expression, and wild redundancy of life, would, so far as superficial appearance is concerned, give being and energy to the frail fabric of man.

The filaments of which the cellular tissue is composed, are nothing more, according to Hunter, Cruikshank, Mascagni, Bichat, Chaussier and Desgenettes, than a mesh-work of absorbent vessels. The experiment performed by Chaussier, which consisted in inserting a urinary calculus into the cellular tissue from which it was entirely removed by the absorbents, we particularly mentioned on a former occasion. This tissue, as exhibited to Fontana in his researches, appeared to be exclusively composed of a congeries of very tortuous vessels, many of which presented the knotty and valvular appearance of lymphatics. By Bichat this tissue is considered the origin of the absorbent system, which together with the arterial absorbo-exhalents enter

very largely into its structure. Numerous pathological phenomena disclose the character of this organ in a very conclusive manner. Thus the astonishing rapidity with which it segregates the humidity from the atmosphere;—the removal of fluids from its cells deposited in them either by disease, accident or intention;—the introduction of blood into it from mental emotion, friction, or exposure to a current of cold air, which would not have been admitted in its normal state, are all phenomena which cannot be satisfactorily explained if we refuse to admit the existence of absorbents in it. "Oftentimes in phthisis" says Bichat, "the ulcers are suddenly emptied, and we find in the subject who dies immediately, only the place which was occupied by pus or sanies; I have already known two patients to die in this way by a re-absorption almost instantaneously and exactly analogous to external ulcers."

Muscular Tissue. By injections and the microscope it has been demonstrated, that the muscular fibres are very delicate canals. This by some anatomists is denied, but Lecat, Verheyen and Vieussens are very decided, and believe it legitimate to conclude from their observations, that each muscular fibre is an assemblage of vessels of a particular order, continuous with the arteries and veins, but placed out of the circulation of the latter. The existence of these vessels is admitted by Mascagni and also by King who considers them blood-vessels. By Tauvry, Sanguinetti, and Bernouilli they are supposed to be very minute arteries, and Teichmeyer attempted to prove this fact and succeeded at least in proving the muscular fibre to be tubular. We are also assured by Lancisi, Cowper and Willis that they have injected them.

The researches of Blancard and Muys deserve particular consideration. The former tied the crural artery and vein of the thigh—incised a large muscle longitudinally without an effusion of blood. This he attributed to the parallel situation of the fibres which shielded the blood-vessels from his knife. The muscle was then cut transversely and immediately there was an abundant discharge of blood. This induced him to believe that the blood circulates within the fibre. In another experi-

ment he separated the muscle from the body—incised it longitudinally—injected into the arteries a blackish liquid and saw it pass from the arteries into the muscular fibres. After having repeatedly injected the crural artery of a lamb, with warm water, Muys saw the muscular fibres lose their colour and become perfectly white. He injected the same vessel with a coloured fluid and filled not only the smallest vessels, but distinctly observed also, that it passed through each fibre, in a serpentine or undulating direction. He moreover, saw the smallest vascular branches which had escaped observation before, thickly scattered around the fibres. Examining attentively with the microscope, he was well assured, that the smallest fibres were filled with the injection, though he could discover no evidence of it in the interstices.

The fact being established, that the muscular fibres are vascular, the nature of these vessels next presents itself for investigation.

The structure of the muscular fibre appeared to Leuwenhoek vesicular. Hook adopted this opinion: it was employed by Creone as the basis of his doctrine of muscular contraction, and by Keil and Stuart for the same purpose. Muys says the muscular fibre is composed of fibrils which are sometimes cylindroid and sometimes knotty, and with regard to the latter appearance repeated observation convinced him that it really exists. These knots were recognized by Blancard but he considered them valves or contractions, and by Lecat they were compared to a string of beads. By Swammerdam, De Heyde, Cowper, Ruysch and Borelli it is asserted to consist of a series of globular rhomboidal molecules. By Mr. Bauer, Sir Everard Home, Prevost and Dumas, which has been recently confirmed by Mr. Milne Edwards and Dutrochet, they are said to be globular. There is no essential difference in these numerous opinions ancient as well as modern; they uniformly assert the primitive fibre to be knotty or globular, presenting of necessity a superficies more or less irregular, a circumstance which corresponds precisely with the appearance of a valvular absorbent vessel.

The cellular tissue which we know abounds with absorbents, and which enters largely into almost every organ, is very abundant in the muscles. Thus Bayle and Hollard inform us that they contain this tissue; Bichat says "I know of no system that has a greater proportion of it," and Dr. Horner remarks, that every muscle is surrounded by an envelop of it, called by Haller membrana musculorum communis, which at different points of the body exhibits various degrees of condensation. By Albinus, Haller and Lecat it is affirmed that the nervous branches which penetrate the muscles are dissolved in entering and are transformed into cellular tissue.

As there are those who may perhaps be inclined to believe, that muscular fibres are small arteries, the following considerations will serve to remove their doubts. If muscles were continuations only of blood-vessels, insects could possess them, for it is known that they are amply provided with the former, but are destitute of the latter. Colour, moreover, is not essential to the constitution of a muscle, for in fowls, in amphibious animals, in fishes, in worms, and in insects, through all the gradations of animals of different species, or of different sizes, the colours of the muscular fibre change. This circumstance has been evinced in the more perfect animals by Gorter; for he found that if a warm fluid be injected into a muscle, the colour is removed, but its power of motion is not impaired. That the blood has no agency in imparting its particular colour to the muscles is evinced by the fact, that although some animals, as frogs, have white fibres, they have red blood. From Bichat, we learn that the colouring matter of the blood, and the fibrine which circulates freely in it, is out of the course of the circulation when combined with the fibres. In asphyxia, Bichat, Horner, Bayle, and Hollard inform us, that the colour of the muscular fibre remains notwithstanding the changes which the colour of the blood experiences. It is conclusively proved, then, that the fibrine or colouring matter is not violently driven into the muscular fibre by the impulse of the heart or deposited there by the capillary arteries, but by an order of vessels of different organization, and of different

properties, which originate, as we shall prove, from the capillary arteries.

Contractility is without contradiction, the most conspicuous property of every organ into which the muscular fibre enters as the chief element. This property exists in an eminent degree in the absorbents.

Muscular contractility remains for several hours after death. This is particularly the case with the absorbents, for they are known to perform their functions some time after dissolution, and in the execution of this office we have positive reason to believe it is done in virtue of a species of contractility.

When an individual dies suddenly in consequence of violence, or apoplexy, the irritability of the muscles continues for a much longer time than under ordinary circumstances. This is likewise the case with the absorbents, for no practitioner has ever had a case of apoplexy, without remarking, how singularly tenacious the dead body is of its heat, the production of which we shall prove is to be attributed to the absorbent system.

In persons who die from the action of deleterious gases, such as carbonic, the sulphurous, the hydro-sulphurous, &c. the power of muscular contractility promptly disappears. This is the case also with the absorbents; for the irritability and sensibility of the whole system being utterly and instantaneously destroyed, there is no evolution of heat—there is no absorption of stagnant fluids from the cells of the cellular tissue as frequently takes place under other circumstances.

The muscles become diseased. Thus the heart which is undeniably the most powerful muscle of the body may in part become ossified. A case of this kind is on record by Morgagni. In the muscular substance of this organ, Bonetus found a deposition of earthy matter. The valvular apparatus between the auricles and ventricles, which is muscular, we are told by Baillie is liable to the formation of bony and earthy matter in it. Beclard says, he has found osseous transformations of a knotty appearance in the muscles. The muscles abound in azote more than in other animal parts; and the absence of this principle, from

the combination of hydrogen and carbon, which exists during health, entirely converts them, we are told by Hensler, under the influence of Elephantiasis into an adipocerous substance resembling soap or spermaceti. This pathological nutrition evinces the existence of vessels for the performance of that function, and consequently presupposes vessels capable of removing the fibrine before these morbid transformations could be accomplished.

Pulmonary Tissue.—That absorbents abound in the respiratory apparatus the following facts will serve conclusively to evince. By Cruikshank we are informed, that next to the liver, the lungs are the part in which he has found the greatest number of absorbent vessels. The absorbents of the lungs, he says, are divided into two sets, a superficial and deep seated. The superficial set, like those of the other viscera, are not always to be found. He was able, at one time, to cover the whole external surface of the lungs with the absorbents he had injected; and at another time he was not able to find one. He has frequently remarked the same fact with regard to the liver. The experiments of M. Poillet, a French Military Surgeon, forcibly illustrate the anatomical structure of the lungs. Besides several others, he instituted some experiments to ascertain how water which penetrates the lungs of drowned persons is removed, and he found, that it is accomplished by absorption. He submersed several animals in a solution of prussiate of potash, and was able to ascertain the presence of that salt in the pulmonary veins, in the left cavities of the heart, and in the whole arterial system. The experiments of M. Segalas strikingly support the same fact. He ascertained that all the symptoms of intoxication were produced by the injection of alcohol into the veins, and by injecting it into the bronchiæ the same phenomena were developed. To the same effect, the experiments of Professor Meyer might be adduced.

Dr. Daingerfield tells us, that mercurial ointment is not absorbed by the skin, consequently we are led to inquire, how salivation is produced when it is rubbed upon the dermoid organ. Many circumstances induce the belief that this is accomplished

through the medium of the pulmonary apparatus. Thus, it is a fact that many persons have been salivated merely by breathing the air of venereal wards in different hospitals which contained patients under the use of mercurial ointment. The effects of breathing an atmosphere tainted with mercurial effluvia, is beautifully illustrated in a case recently noticed in a Western Medical Journal. A gentleman was requested to shave one of his friends who was at the time profusely salivated. The breath was extremely offensive and he was compelled to inhale it for some time, as the operation was tedious. Though in perfect health, in three days a sore mouth, a copious spitting and disagreeable breath announced a true and profuse salivation.

Mucous Tissue.—The structure of the villi of this tissue is a point which has been much contested. We know what was the vesicle of Lieberkuhn; this anatomist having observed that the intestinal villi were swollen by injection, and that the air driven in diffused itself in them and gave them a spongy appearance, thought that he might conclude from it that they were in a great measure formed of a kind of cellular vesicle, in which the vessels terminated, particularly the lymphatics. Hewson says these villi are entirely composed of a net-work of vessels, of a sort of plexus similar to that which forms the erectile tissues. Alb. Meckel says the lymphatic vessels may arise from these villi which he compares to that which constitutes very young vegetables at the period of their developement.

It is asserted by Lewenhoek, Hewson, Hunter and others, that the orifices of the absorbents are visible on the surface of the villi, though this is denied by Beclard and J. F. Meckel. The weight of testimony however is decidedly in favour of the former, and though there is a difference of opinion as to the number of these orifices, many distinguished anatomists are satisfied of their existence. Thus, Bluland and Hedwig allow but four; Cruikshank and Sœmmering have seen from six to ten.

That absorbents exist in very great numbers in the mucous tissue is proved by the absorption of chyle and of watery drinks from the intestinal canal. To determine whether the veins

perform the function of absorption, Hunter injected solutions of musk, mixtures of indigo and starch-water in the bowels of various living animals, but upon examination no one of these substances could be discovered in the mesenteric veins, but were found in the absorbents. We may also infer the absorbent nature of this tissue from introducing into the bowels those substances that are strongly odorous or sapid and which may be afterwards found in the lymph. We are told by Dr. Begbie, that a suppository of datura stramonium was introduced into the rectum to allay a painful disease, which was dissolved and entirely absorbed. The patient became affected with symptoms closely resembling delirium tremens which was near destroying him.

Wherever a mucous membrane is found there we are sure to find absorbents in sufficient abundance. Thus we observe the venereal virus absorbed from the glans penis and from the urethra; the serous part of the bile from the gall-bladder; the more fluid part of the urine where it remains too long in the bladder, and there are cases on record of obstructed urethra where the urine has been almost entirely absorbed.

Dermoid Tissue.—The existence of absorbents in the skin has been incontestibly evinced by Lauth and we regret that we have not had an opportunity of seeing a circumstantial account of his labours. A multitude of indisputable facts, however, independently of anatomical evidence, satisfactorily demonstrate their existence. Thus, it is a circumstance perfectly familiar to those who follow the seas, that thirst almost insupportable becomes much relieved by putting on a shirt dripping with salt water. We learn from Dr. Simpson that the water of a bath was considerably reduced a short time after the legs of a phrenitic patient were put into it. The anti-diluent plan of treating dropsy not having any controul over the accumulation of water, De Haen declared that the obstinacy of the disease was to be attributed to moisture absorbed from the atmosphere. Professor Home found himself heavier in the morning than he was just before he went to bed the preceding evening, though he perspired all night and had received nothing by the mouth or in any other sensible way. Mascagni kept his feet in water

for several hours,—observed a painful swelling of the inguinal glands and a transudation of fluid through them. He was seized with a defluxion of the head and had a constant discharge of a salt and acrid fluid from his nostrils. A lad at New Market, we learn from Watson's Chemical lectures, was almost starved to reduce him to a proper weight to ride a race; he was weighed at 9 o'clock in the morning, and again at 10, and in that period it was found that he had gained near two pounds and a half though he had drank in the interval half a glass of wine only. By Cruikshank it is said that an individual who laboured under stricture of the œsophagus received nothing either solid or fluid into the stomach for two months; he was exceedingly thirsty and complained of making no water. The warm bath was administered for an hour morning and evening for a month, which allayed his thirst and he was enabled to urinate as in health.

Numerous articles, such as mercury, sulphate of quinine, squill, &c. have been absorbed according to very high authority through the skin. This plan of endermic medication as it has been denominated, was I believe originally suggested by M. Lembert, but M. Bally has more recently pursued the investigation on a larger scale, and arrived at results more conclusive and practically useful. Thus the salts of morphine quickly displayed their influence upon the brain and nervous system; belladona produced extreme dilatation of the pupils and diminution of vision; strychnine affected the locomotive system without much disturbing the sensorial functions, and the deutochloruret of mercury cured old syphilitic complaints by the endermic application.

[As the arrangement of several of the facts may appear awkward, I will advertise the reader, that they were made known to me since the essay was written, and wishing to incorporate them with it, I have included in crotchets all the facts of this character.—That the skin possesses an absorbing faculty is now placed beyond all question by the experiments of M. Martigny. He placed upon the palm of the hand an inverted glass funnel filled with water, and remarked an obvious dimi-

nution of the water in the tube of the funnel after an hour or even less. He then varied this experiment, using precautions to preserve the pressure of the hand on the funnel uniform, as well as to prevent the evaporation of the water, and still observed a sensible diminution. The following very simple experiment gave equally pointed results. In two small vessels of the same form and size he placed equal quantities of water, one being placed at his side, exposed on the surface to the air, while into the other he put his two hands for half an hour. He then removed them and dried them in a handkerchief, the weight of which was taken both before and after this operation. The water of the two vessels was likewise weighed, and the proper correction being made for what was removed by the handkerchief from the hands, he found that 178 grains had been lost by absorption. He placed a few drops merely of the fluid to be examined on the inside of the arm, covering them with a small watch-glass, and allowing the experiment to go on for eight or twelve hours and upwards. In this way he remarked that water and brandy were absorbed altogether, and wine, beef-tea, and milk almost entirely.—From the experiments of Dr. Westrumb, he concludes that the skin is endowed with an indeterminate faculty of absorption: it can admit and introduce into the circulation all sorts of substances, from the least to the highest degree of fluidity, provided they are soluble.]

Osseous Tissue. The cases of fracture recorded by Mr. Abernethy, which, after perfect union had taken place, were refractured by the absorption of the callus; a number of diseases of the bones, particularly mollities ossium, and the impregnation of them with madder, evince most satisfactorily the absorbent nature of the osseous tissue.

With regard to the absorbent character of the other tissues of the body, a reference to almost any system of anatomy will furnish the reader with the most ample and satisfactory evidence. The obligation we are under to consult as much as possible the utmost brevity prevents us from examining this evidence in detail.

[TO BE CONTINUED.]

ART. V. *Observations on the use of Tartar Emetic in certain states of the System.* By JOHN R. LUCAS, M. D. of Mooresville, Alabama.

DR. CARTWRIGHT, in his essays upon Pneumonia Biliosa and Malignant Bilious Fever, published in the American Medical Recorder, has ascribed to tartar emetic, the possession of *active stimulant and sedative* powers over the human system.

That tart. emetic does, in its action upon the system, possess, apparently, these opposite properties, according to the particular state of the system at the time of its administration and operation, is most true: but that it does, in point of fact, really possess a general stimulant property, is, I think, much more than questionable.

With the Doctor, I believe it possesses, and is governed by its own never varying law. I believe its action is the same in kind in all states of the system; and that this is uniformly in its effects what medical men understand by the term sedative. Whilst I believe this, I also most cordially approbate its use in the states of the system, in which Dr. Cartwright employed it; but upon a view of its controuling effects over the system, distinct from any supposition that it produced good in his hands, by the exercise of any stimulant quality whatever.

This acknowledgment, upon a superficial view of the subject, appears to constitute as singular a paradox, as do the effects sedative at one time, and stimulant at another, ascribed to the agent by the Doctor. But upon a close examination of its properties, and the conditions of the system referred to, we shall at once be enabled to account satisfactorily for the success of a practice, founded in part upon an erroneous view of the properties of the agent employed.

The cases and conditions of the system referred to by Doctor Cartwright, were *all* characterised by an *increase* of action in-

tent or open. The difference in the symptoms was occasioned not by a difference in the kind of action present, but grew exclusively out of the extent to which this action progressed in each individual case; and hence, the therapeutic agent relieved the *apparently* diverse, states of the system, by maintaining the same uniformity of action in every state, and thereby effecting in all cases the same pathological changes in the condition of the system itself.

I would here say that the action of tart. emet. was never stimulant to the general system, but that its effects were invariably and uniformly sedative, under all the ever varying states of the system. We all know that it is proscribed in fevers of a typhoid character, and indeed in all diseases, the character of which is that of low action.

That in cases of suffocated excitement, as well as in cases verging to this, tart. emet. should exert an apparent stimulant effect upon the system, which upon a slight view might pass for real, is easy enough to explain, by referring the result of its effects to its relaxing and sedative properties. Thus the heart being acted upon through the effects peculiar to the agent, experiences relaxation and diminution of its action. The blood vessels, in consequence of their direct connection with the heart, feel the same relaxation or diminished action throughout, of which all their branches partake in due proportion; thus particular portions are relieved by the general effect exerted upon all; hence the lost balance, the cause of the excitement, is restored; excitement, during this pathological change, is evolved or changed from a latent to a sensible state; and reaction is established. In this point of view, the lancet itself may be ranked with the class of stimulants; but surely it cannot, and will not hence be inferred that this state of things is, or can be brought about by any direct stimulating property in *either* of the agents themselves.

Again, when the excitement is already too high, open and free, the tart. emet. and the lancet reduce it upon precisely the same principle, as in the former condition of the system, just above stated; viz. the relaxation consequent upon their use places the blood vessels of the system in general, and of the secretory or-

gans in particular, in that state and condition which enables them to discharge their proper functions. By an increase of secretion the fulness and tension of the vessels are removed, excitement abated, and the force of the circulation reduced. The degree of relief in either condition of the system will depend, exclusively, upon the extent to which the system and different organs involved, may have been implicated, and their susceptibility to be acted upon at the time.

Acting under the direction of these principles, I can bear testimony to the success of the practice, and I would respectfully suggest its more extended use in those torpid cases of the bowels, which unfortunately, we but too often meet with, and which have heretofore defied the usual routine practice of the day.

If I have misunderstood, or misrepresented the opinions of Dr. Cartwright, I would beg leave to assure him it is not the effect of design. That my remarks have truth alone for their object, and the guarding from error those who, without his tact in prescribing, might under the supposed sanction of his name, resort to the use of an agent so powerful in its controul over the system, in cases totally contra-indicating its use.

ART. VI. *Case of Hereditary Consumption cured.* Communicated in a letter to one of the Editors.

THE following is the statement you requested me to make. I hope that its *minuteness*, which seems to imply too much egotism, will be attributed to the strong conviction, (however correct) which the writer has of the importance of treating almost all complaints of the lungs, under certain modifications, in the way here stated;—from a cough, arising from the early stages of a common cold, to the most alarming symptoms of consumption. I am, however, aware that there are exceptions to this *general* rule, for I have known one instance myself, where the most violent expectoration of the worst kind, with fever and occasional dis-

charge of blood from the lungs, was cured by great quantities of opium, bark, and the greatest quantity of food, (the appetite was excessive) and constant riding on horseback,—though when first placed on the horse the patient could not move more than ten rods before he must be taken off, but such a case is very uncommon. When such cases do occur, I believe it is from *wrong* treatment at first. I hope it will also be taken into consideration, that I have seen almost all my relations, and many of my friends, sink under a treatment exactly the reverse of what I myself experienced,—and that the physician, who was at last the cause of my being saved, insists strenuously, that a consumption is not necessarily incurable, because it is hereditary, and that a *belief* that it is so has been the cause of more deaths by that disease, than almost any other. And that instead of prevailing as it does, it would be rare, if common colds were not neglected till they became disease, and were not then still neglected or made worse by ill treatment, for many months after they might have been put an end to by four weeks of confinement, extreme temperance, small bleedings, and moderate blisters. This, however, is to be understood of disorders which are not complicated, or are only connected with diseases of a bilious nature, for in such cases the same treatment is often equally good for both. My father and his sister were both consumptive from the age of sixteen. The one kept the disease under till after fifty, and then lived in comfort till seventy and died of [illegible] complaint. The other kept it under by going to sea twenty years of his life, and died at sixty from another cause. One of my two only sisters (I had no brother) died of a complicated consumption at the age of twenty-four; she had difficulties peculiar to her sex, which rendered all practice for the principal disease unavailing. On my mother's side of the house, nine of my cousins out of two families, leaving but two, died under thirty years of age, of regular, unmixed consumption. They lived at some distance from our family, though I saw them two or three times a year, but they had an unfortunate prepossession that a *hereditary* consumption was neither to be kept off, or cured, (their mother had died of it) and consequently, they eat, drank, visited, were exposed to the cold, to wet feet,

hot rooms,' and thin clothes. Cough, fever, and even blood-spitting, were with them no reasons for precaution, but rather for increasing their means of enjoyment, and the gratification of their appetites, as *their time on earth was to be short.* This was their honest belief, and as honestly practised upon, to the last moment, both by the males and females. Our different opinion had no weight; the sacrifices a conformity to mine would require, were *too great* for the mere *possibility* of longer life. To live a life of *starvation, care* and *watchfulness* was *not* living.

For myself, I was consumptive from the age of fourteen till past thirty, but by the direction of an excellent medical man, always kept it under, by *great* temperance, sometimes for three weeks, and sometimes for *three months*, with occasionally bleeding and blisters, and when the disease was not actually upon me, using much exercise in all weathers, but great care not to take cold. From that time till my thirty-ninth year, though with a slight habitual cough, I had been so well as almost to forget that I was subject to any danger. I had continued when there was occasion, which was rare, to pursue this plan without consulting any one. In this situation, *without any fever*, pain, *blood-spitting*, or *difficulty of breathing*, all or some of which symptoms had attended my former attacks,—my common cough began gradually to increase and became at last exceedingly troublesome, but I felt on every other account perfectly well. From a strange infatuation, the possibility of this being my old enemy hardly occurred to me. The cough became insupportable, fever came on, my strength and flesh wasted fast away, but I had no pain; I grew worse every hour, and at last insisted upon being bled, and blistered. I was much relieved from the cough after the second blister, and immediately following it, came out on my breast a tumor as large as a red pepper, which had to be opened *very* deep, and discharged the most offensive matter. This was allowed to close, and my fever and cough gradually *increased*, as the external discharge decreased. I bled almost every day at the nose, but raised no blood from my lungs, and still felt no pain, but I was reduced almost to a skeleton, and my cough was so suffocating that I expected almost every day to breathe my last. I had many

strong bilious symptoms, and occasionally took an emetic, and my bed was wet through every night with perspiration. I had now no appetite, *but because I had not*, took large quantities of the most substantial food, with wine, bark and opium!!. Indeed I was weak enough to act like an idiot, for I ought not, feeling as I did that it was wrong, to have submitted to the persuasions of others. *Two days after* the external discharge from the wound on my right side *entirely ceased, the chill which preceded* the afternoon fever, was much more violent than it had ever been. The paroxysms of coughing became absolutely insupportable, (the fever was raging,) I was completely suffocated, and my senses left me. When I opened my eyes, I found that in the struggle I had burst a large ulcer in the right lobe of my lungs, and the matter and blood had been thrown out, and I now continued to throw it up abundantly. I had watched my sister through her whole complaint and seen her do this same thing, and with the same struggle for life repeatedly. My eyes were now opened completely, and I looked back with perfect astonishment upon my folly, but supposed it too late to retrieve it. I had had no internal pain, but it now came on with extreme soreness; another ulcer formed immediately, in the left lobe of my lungs; the stabbing pains, and soreness were soon so great that it was with the greatest difficulty I could bear to be turned in my bed. In this desperate state there was no resource, it was thought, but to keep *up my strength.* But this was adding fuel to the flame that was destroying me, and I sunk as rapidly as possible, discharging a pint of matter every day from my lungs. Several times each day, my struggles were so great and so shocking to witness, that all those who were not obliged to be present, would fly from the house. The struggle would at last cease, the blood would no longer circulate in my limbs, I would perceive them become numb and helpless, my sight would fail, then my hearing and for a few moments my senses would be entirely gone. I would again open my eyes, perceive my friends around me; and know *that this was not death.* During this state of insensibility, the load of blood and matter would be forced from my lungs, and I would now breathe again. In this situation a physician was consulted who said there was

one only hope; that an entirely different treatment, carried to the *greatest extreme* might *possibly* save me, at least it would relieve me from the raging fever, and the dreadful cough, and danger of instant suffocation which every hour seemed more certain, and that I should at least sink more gradually. We knew that the prospect before me was death in a few days; my friends therefore united with me in telling him to give his orders, and they should be implicitly obeyed, for no treatment could place me in more immediate danger than I then was.

The complaint began in October, it was now the beginning of January. He regulated the air in my rooms, both night and day, by a thermometer, and kept the temperature at fifty-five through the whole twenty-four hours. This was rather too cool to be agreeable, but if hotter I was in a raging fever, if cooler I could not breathe. In the first four days after he began the treatment, he bled me *three* times, and took away all my bark, wine, opium, soup, beef and coffee, under which, (by way of gaining strength) I was sinking like snow before the sun, and substituted for *all* these, only *three small* teacups full of milk and water in twenty-four hours, with bread dried in an oven, three wine-glasses full of tea of Iceland moss, and a cup of weak Hyson tea, morning and evening. I took a small pill of cicuta as a substitute for the opium which had never checked my cough, and I doubt whether the cicuta did me any service. He also *covered* my breast with small blisters, putting a new one on every twelve hours, till I was fairly covered from shoulder to shoulder, these were constantly succeeding each other, so that I frequently had six in different stages, dressed in the twenty-four hours, and for three months I was never without them more or less; and for *two years* never three months without an occasional blister, and sometimes would be obliged to wear them moderately for four weeks, when abroad *apparently* well. The first blisters were not found rapid enough in their work, and in addition to them, on the very spot where one had been drawn, over the most painful place, within five days after this treatment began, he made so deep a sore with sulphuric acid, that I could almost see my ribs every time it was dressed. I took just rhubarb enough every night to move me

very gently in the morning. As I bled every day at the nose, the lancet was less necessary, and it was some weeks after the third bleeding, before I resorted to it again; but I was bled thirteen times in the course of a few months, and more than I can number in the course of the two following years. For many days after the severe treatment began, I had not one single favourable symptom, and *looked* much worse, as all my colour was gone except in the height of the fever, and the bones almost pierced through my skin. I constantly expectorated the worst kind of matter, with small quantities of blood, and the hard tops of tubercles of a chocolate colour, and as offensive as possible. I was on the point of suffocation every moment that I did not set up, and consequently could not sleep more than two or three minutes at a time; with such naps I usually groaned and struggled through the night. The doctor never left me for an hour, till near morning, and was by my side all day. He would watch me, putting off or on the clothes, regulating the fire, my blisters, or my position in bed; and would not trust me to the watchers, till near morning. During the day he would place me on an elastic board, or wooden horse, and have it moved gently, but I could not bear it five minutes, (though in six weeks after, I could exercise myself upon it at intervals three hours in the day,)—he would call in my servants and make them rub every limb, several times a day, and would take his own turn at this exercise, and oblige every gentleman who was permitted to see me, to do the same,—but except an increase of appetite, upon which I was allowed a little more milk, no amendment appeared, indeed the prospect became darker every hour, all who saw me pronounced the case hopeless. I often supposed that I had seen the sun set for the last time; my nights were dreadful, and the days almost as bad; every breath was drawn with an effort and my strength rapidly failing under the exertion. I still bled from my head every day, my *bed* wet *through* every night, and my *clothes*, *if up*, all day, from mere weakness, except when burning with fever. The Doctor still said that many were lost by giving up the point and that whilst there was life, there should be always hope, but hope was gone to all but him. On the 18th day after the severe

treatment commenced, at the hour when the afternoon fever was expected, and when death always appeared immediate—the fever kept back half an hour; when it came, it was more moderate, the cough less distressing, the expectoration less, and through the following evening I breathed with more ease than I had for two months. I was able to sleep without suffocating every five minutes, and from this hour I gradually recovered; at first by the slowest degrees, but in a few weeks most rapidly. My appetite now became insufferable, but the least excess even in my simple food, obliged me to be bled, or increase my blisters, or both. Mere bulk, even of liquid in my stomach, increased my cough &c. I therefore endeavoured to have my food as nourishing as possible, and in small quantities, without being stimulating. I therefore used, after some weeks, milk *without* water. Then boiled the milk with *bread in it*, but always used it cold, and increased the quantity as I could bear it. I never drank, to check the cough, more than a teaspoon full at a time, and rarely that, preferring to keep a piece of hard rock candy in my mouth, which would not only allay in some measure the irritation, but was a nourishing food, for my stomach was from abstinence now in such good order, that it would *dispose* of whatever went into it, though the lungs would give immediate intimation of too much, by pain, fever and cough.

In March the ulcers all closed. I at last exercised moderately in a close carriage with the glass partly down, and in the course of the summer travelled a great deal, but was obliged to do it with every accommodation, as I could not bear the cold, the dampness or the wind; the least motion made me perspire profusely, and every night I did the same, till September. If this was checked by cold or light covering, pain, fever and cough would return. I was therefore obliged to endure it, and let it wear out. Through the summer I eat milk, vegetables and fruit (especially that which had undergone the operation of fire,) and, in such quantity as would prevent my suffering from hunger. I also drank tea, which refreshed without inflaming me. During the first two months of my severe treatment, I suffered every moment from hunger. I gained so fast all summer, on my milk and

vegetable diet, that I was frequently obliged to desist from it, and live a day or two on weak tea and bread; for I had such a tendency to make blood, that if I did not do this, I found it necessary to bleed. In some instances I would live *six days* on tea and bread, even when strong enough to travel. For more than a year an ounce of meat with a very little wine would produce such stabbing pains, difficulty of breathing and high fever, that I would be obliged to be bled largely and blistered, and starve for ten days before I could get the fever under. I was confined to the house for about two months the winter following, with more or less indications of the disease, and through the *second* summer and winter, I contended in the same way, though with constantly increasing health; and even when apparently well, there were certain positions of my body in which I could not place myself, on account of the tenderness low down in my lungs where the worst ulcer had been on my right side; and that is still the spot where the pain always begins. For some years this would always take place and fever follow, if I allowed a little irritation at the lower part of my windpipe, arising from a slight cold, to increase. But I at last discovered that the slight irritation which was often a prelude to very serious evils, and to weeks of bleeding, blistering, starving and medicine, could be put an end to by placing a small blister not much larger than a twelve cent piece directly in the hollow of the windpipe, at the bottom of it, and keeping housed, and living low two or three days, *if in winter*. It is now *eleven* years since my severe attack. For *two*, I eat no meat, on the third very little, on the fourth I lived much as formerly, using wine and meat, but desisted when finding it necessary. I was occasionally bled till within the last two years, when I have made no use of the lancet, and am in better health than I have been for twenty years. I live *generally* as others do, except in the use of wine which I think agrees with me less than it did some years back. It produces no consumptive symptoms, but makes me bilious.

I go out in all weathers, both in winter and summer, and do not take cold *once*, when all my hardy neighbours do *ten times*; merely because I take care to guard against it. If I am caught

with a cold, I immediately leave off my usual food, and keep housed, whether winter or summer, and in a few days it is gone. Even when in perfect health, in winter, (though I go out in a storm,) I do not suffer myself to go even from room to room in the house, without something over my shoulders and on my head; and in the warmest weather in summer, I never set where the wind can blow on me. *These precautions* appear absolutely ridiculous to all but my indulgent friends, but *with them*, (the precautions,) I can keep *well*. I travel a great deal, visit mountains, rivers, lakes, go through much fatigue, am in society as much as I wish, take care of the sick, go without rest or sleep, and by eating and drinking less rather than more, (as is common under such circumstances,) I generally come out from any such trial of danger, of fatigue or anxiety, much better than those of my associates or friends, who are supposed to have sufficient hardiness to contend with any thing. Since my recovery, many others have, by the same means, been restored from different stages of lung complaints (from what they call *only* a hard cold to serious and consumptive complaints,) who were all growing worse under a stimulating treatment. Some of my friends, (healthy persons,) who were subject to violent colds several times in the year, which would last them many weeks, have by my persuasion adopted *warmth and starvation* if the weather is cold, and if warm, *only* the starvation, and cure themselves in five or six days, and some of them say that they *no longer* take cold, as they begin the regimen on the first intimation, and in two days they find *that they had no cold.*

One remarkable thing in my complaint, was, that I had none of the pain in my side to which I was subject when young, till after the first ulcer discharged, and that I spit but little blood from my lungs, though so oppressed with blood. One thing about blisters should be guarded against: after I had been well for more than a year, I had an attack, which bleeding and blistering put an end to, and I had no fever, cough or pain for five days, when, to my surprise, a *constant* fever came on, but *no pain or cough; the fever had no intermission.* In my impatience to get well, I had been bled and covered my whole breast with one large blister, and

to make it more effectual, had dressed it with savin ointment. It *had cured me*, but it had now got into such a bad state, loaded with matter, that the matter was absorbed, and produced the fever—we put one on my arm, that the change might not be too sudden, and cured the bad one as soon as possible, and the fever then left me immediately. It is best to have a constant succession of new blisters, and never let any of them become very sore.

(The writer of the above case is a man of unquestionable integrity, and what is stated may be entirely relied on. He is personally known to the writer of this note.) C.

ART. VII. *Case of Hæmatemesis, cured by the use of Purgatives.* By Dr. SUTTON I. HARRIS of Oak-hill, Frederick County, Virginia.

ON the 22nd. of July 1828, I was sent for in great haste to visit a man about fifty years of age, of rather a lean habit, who had generally enjoyed good health, until a short time previous to this date. I arrived at his house about 4 o'clock in the morning. I found him vomiting very dark blood, partly coagulated, in very considerable quantities. He was puking into a vessel, in which there was about three pints of blood; and the persons present informed me that they had emptied the vessel of at least six pints before I arrived. He had considerable pain in the head, pulse very small and frequent, extremities cold, tongue foul, and on raising his head from the pillow syncope was produced.

This man had worked hard in the harvest field for ten days, during which time he had used spirits freely. He informed me that he had felt, for several days, some sickness at the stomach, and had occasionally vomited bilious matter.

Taking the circumstances of this case into consideration, I was led to the conclusion, that the vomiting of blood was entirely symptomatic; that all the symptoms of the case plainly shewed that the interior veins were overcharged with blood,

and the only means of relief was to lessen that fulness by evacuating through the liver. To attempt to arrest the hæmorrhage by styptics was impossible; for any article of that class of medicines would have been thrown up as soon as swallowed. I therefore, gave him immediately forty grains of calomel, in the form of pills, and applied a blistering plaister over the stomach.

I saw the patient again at 11 o'clock A. M. He had not vomited since he took the pills; his pulse was fuller, stronger and less frequent; the plaister had drawn. He said he felt as if he should soon have a passage.

I saw him again at 7 o'clock P. M. He had had several large black passages, which had somewhat the appearance of coagulated blood, but was evidently mixed with bile; for when the white clothes which had been used, were slightly washed, a deep green stain was left wherever the matter had touched them. These discharges continued until about 12 o'clock at night, when he felt some pain or griping in the bowels. His attendants gave him a few drops of laudanum and he slept the balance of the night.

On the morning of the 23d. he was quite easy; could sit up in bed; tongue still foul, pulse small, yet larger than before: I gave rhubarb, aloes and calomel, each 10 grains.

Saw him again in the evening. The medicine had operated well; discharges black bile, which continued for several days, with the aid of six grains of rhubarb on one accasion. The evacuations from the bowels gradually changed from black to green, and from green to a yellow colour, and his strength increased as the discharges approached a natural appearance.

This man has been in as good health as usual since that time, until the 15th of July 1829, when he was suddenly attacked with a dizziness in the head, and in a short time the whole system was in a kind of palsied condition, except that there was rigidity of the muscles. These symptoms gradually going off, his pulse became very active. When I was called to see him, I found him with a strong full pulse, hot skin, and his vision much impaired. I drew about two pounds of blood, and gave him jalap and calomel, each twenty grains. The bleeding re-

lieved him completely: the medicine operated well, and he resumed his ordinary business in two days after, and has remained in good health ever since.

ART. VIII. *Case of Lithotomy.* By HUGH HUGER TOLAND, M. D. of Newberry district, South Carolina.

[NOTE. Lithotomy has of late become so common as almost to have ceased to be regarded as one of the capital operations of Surgery. Yet we acknowledge that it is with much satisfaction that we hear of its frequent and successful performance by young surgeons who have just left our school; and in this spirit we give publicity to the following case, of which our young friend has much and just reason to be proud. Dr. Toland is only 22 years of age, and is the first to have performed this operation in that part of the country. EDS.]

IN September, 1828, Davis Motes, a lad aged seventeen, who had suffered with the stone fourteen years, came under my care. Although he had been successively the patient of physicians of considerable eminence the greatest part of the time, his situation was truly miserable. During the paroxysms, which generally continued two weeks, he was incapable of taking any exercise: he had incontinence of urine which passed guttatim, attended with the most excruciating pain. He not only had pain in the perinæum and glans penis, but also in the right and left hypochondriac regions, which were very tender to the touch. His general health was much impaired; complexion sallow with a capricious appetite, and depraved digestion. He had intervals of comparative ease, although the incontinence of urine continued, which rendered his situation at best very disagreeable. Having taken medicine until he despaired of ever obtaining relief thereby, he readily consented to submit to the operation; but being unprepared, it was deferred until the 13th. of the June following, prior to which time he was sub-

mitted to a preparatory course. But in despite of the best directed use of antimonial emetics, mercurial cathartics, and a strict adherence to a low diet, the pain in the left hypochondriac region, with an erysipelatous eruption over the right, remained. Being conscious that to operate on him in that situation would be replete with danger, I gave him calomel alone, until ptyalism was produced; and as soon as the mercurial action was established the local affections vanished: his appetite became unusually good, and the irritability of the bladder was much diminished, being able to retain his urine one half hour or more. Supposing that in a short time, unless he was left under the influence of mercury, he would be in the same situation he was prior to its use, I concluded to operate, and the 13th of June last was the day set apart for that purpose.

The necessary preparations having been made, and the physicians present being satisfied that it was a case of the stone, the operation was commenced. Nothing embarrassing occurred, although the division of three small arteries retarded its progress, but they were secured by ligatures without much difficulty. A gorget of the middle size was used, and it was only necessary to extend the incision in the perinæal fascia, for this was the only part which gave much resistance, or prevented the stone from being extracted with ease, or without doing any violence to the soft parts. As soon as the stone was extracted the hæmorrhage ceased, and he did not loose one drachm of blood after he was put to bed. His convalescence was rapid, until the twelfth day, at which time he had an attack of bilious remittent fever, which yielded, in about four days, to the use of mercurial cathartics; these again produced ptyalism, and it was kept up by small doses of the medicine until the wound cicatrized, which was about the thirtieth day. His knees were not tied together until the end of the first week, which probably was the cause of there being so inconsiderable a quantity of blood lost, after the operation was completed. The stone weighed six drachms and six grs. and was very irregular on the surface, which was the cause of his suffering such great pain.

If I had placed implicit confidence in the directions given by

the unparallelled Cooper in such cases, Motes must have died with the disease, of which he is now entirely cured. He says we should not perform the operation of lithotomy when there is much functional, or any organic disease. I do not pretend to say that there was any lesion of structure in this case, but that there was much functional derangement cannot be denied by any. None of the important organs of the abdomen had performed their functions healthily for a considerable length of time, until compelled so to do by the astonishing power of calomel; and to have operated successfully in this case without it, would have been impossible. I think we may conclude, with the language of a celebrated writer, that in such cases it is the magnum donum Dei.

ART. IX. *Case of Retention of the Placenta, caused by an unnatural appendage to that Body.* By SOLOMON TRACY, M. D. of Gallatin Mississippi.

I was called on 27th August 1829 to visit Mrs. W. a lady about eighteen years of age, of the sanguineo bilious temperament, who had been about eight hours previous, delivered of her first child, at the full period of utero gestation. I found her in rather a comatose and desponding condition, with cool skin and clammy sweat, unequally diffused; yet the pulse was tolerably full and frequent. She complained of no pain, and seemed disposed to give no answer when spoken to. I was informed by the midwife in attendance, that the labour had been painful and tedious, that in her effort to extract the placenta she had broken the umbilical cord, and with it tore off part of the placenta. She had then introduced her hand for the purpose of detaching the placenta, but failed to accomplish it, having found it firmly attached, to use her own words, "to a fleshy substance as large as the head of a child" which could be felt through the walls of the abdomen also. She now became alarmed and desisted from any further efforts to extract the placenta.

I also learned from the lady herself that she had felt the tumour in the left side of the abdomen for some weeks previous to delivery which induced her to believe it was a second child.

Upon examination per vaginam I found the os tincæ dilated, with slight hæmorrhage; the uterus was high up and its contractile powers completely paralyzed. As the parts had been much irritated by the the officious conduct of the midwife, and I apprehended not much danger from hæmorrhage, I thought it imprudent to introduce my hand for the purpose of detaching and bringing away the placenta. A bandage was drawn as tightly around the abdomen as the patient could well bear it, for the double purpose of restraining the hæmorrhage and facilitating the separation and expulsion of the placenta and its appendages: ʒss of the secale cornutum was infused in a pint of boiling water, and a tablespoon full taken every ten minutes until all was given. At the expiration of that time she complained of no pain, but the tumour, felt through the parietes of the abdomen was found to have shifted its position; it was now just above the symphysis pubis. Ol. ricini ℥ij were given; then waited an hour when it was repeated. She now for the first time complained of pain, and the uterus began to feel hard and globular: the pain and uterine contraction continued about ten minutes, when the placenta was expelled with a fleshy mass attached to it, by a kind of gristly neck, that would have weighed between three and four pounds. It was enveloped in a delicate membrane which it derived from the placenta, and was about eight inches in length and four in diameter. Its substance was of a fleshy granulated nature: there were small blood-vessels making into it from the placenta through which it received its nourishment.

The child was as large and healthy as usual; and the mother did well after the delivery of the placental mass.

ART. X. *Case of Spontaneous Combustion of the Human Body.* By CHARLES W. SHORT, M. D.

A CASE has recently occurred in this place which, if it be not strictly speaking one of *spontaneous* combustion, is at least an instance in which a more general destruction of the human body was effected, by caloric, than is to be readily accounted for upon ordinary principles, or from the amount of combustible materials consumed on the occasion. The facts of the incident, so far as they have come to our knowledge, are these, and they are to be implicitly relied on; as the circumstances were carefully observed and noted at the time, and on the spot of their occurrence, by several medical gentlemen.

The subject was a woman aged sixty-five or seventy years, of or inary stature, and common bulk, weighing probably an hundred and twenty pounds; she had formerly been more fleshy, but her late habits had impaired her health, and produced a consequent diminution of her flesh. For many of the latter years of her life she had been addicted to the intemperate use of ardent spirits—the drink more commonly used by her was whiskey distilled from corn.

On Sunday evening the 15th of November 1829, she was seen in a state of intoxication, and in that condition returned to a house, in which she lived alone, about dark. Towards ten o'clock of that night a neighbour, who lived adjoining, perceived an unpleasant odour, as that of burning meat, which induced him to examine his smoke-house, and the premises of the deceased; but the door of the latter being fastened, and the windows closed with a shutter or blind, no unusual light was seen, and no discovery made. At midnight the same neighbour, as well as other inmates of his house, perceived the same odour, but no cause was ascertained for it, until the ensuing morning.

At ten o'clock in the morning, the house continuing closed, suspisions of mischief arose in the minds of the neighbours, and

the door was broken open;—a dense smoke issued from the room, the floor adjoining the fire-place and something upon the hearth were discovered to be on fire; water was thrown upon them, and then a black substance was perceived lying on the hearth, which was ascertained to be the remains of the unfortunate woman; so much consumed however, and so disfigured by the fire, as to be mistaken at first for a stick of wood.

Upon a more particular examination of this body, it was found lying on the right side, lengthwise with the hearth, and about the middle of it, with the face towards the fire; the position being partially flexed, and the right arm extended under the head to the floor. The soft parts of the face were so burnt and blackened as to have lost all traces of a human countenance, but the bones were sound; the back part of the head, however seemed to have escaped the fire, as the hair was undisturbed, and a portion of the head dress was unburnt. The anterior parts of the thorax were entirely consumed, leaving only a few inches of the ribs where they join the vertebræ. The soft parts of the abdomen were also gone quite down to the pubis, and laterally to the back-bone. The contents of the thorax and abdomen were entirely destroyed, except the liver, one kidney and a part of the lungs, which were reduced to a small carbonaceous mass. The stump of the left thigh was projecting a few inches from the hip, but the thigh and leg were literally burnt up, leaving only in the direction in which it laid on the hearth, small fragments of calcined bones, which were easily friable between the fingers.—The foot of this leg was detached at the ankle, and lying on the floor, comparatively uninjured, the shoe and worsted stocking, below the point of separation, being sound, and the skin of the instep only slightly blistered.

The right lower limb was not so much consumed, but the muscular parts of the hip and thigh, together with the calf of the leg were deeply burnt, and indeed reduced to a black charred mass of one fourth their former bulk; the bones were not burnt, the skin on the anterior part of this leg was only blistered, and the foot comparatively sound.

The posterior parts of the trunk were much and deeply burnt

but the bones were not exposed, and embedded in the cavity formed by the scapulæ were the remains of some portion of the dress comparatively uninjured, while the skin and flesh beneath were black and burnt. The right arm which, as observed before, laid extended under the head, was but little injured, being blistered only where it was near the head; the left arm was entirely consumed quite from the shoulder, and not a vestige of it remained.

Anterior to the body and among the ashes of the fire-place was a mass of adherent matter, externally encrusted by the fire, but internally moist and red—the remains, apparently, of blood and alimentary matter. The entire remains of the body would probably not have exceeded the weight of thirty pounds.

At the back part of the fire-place there were the two unburnt ends of a small stick of wood. About the middle of the hearth, in an iron vessel, was a small Irish potatoe, the exterior of which was charred, but the interior white and moist. The mantle-piece, of poplar wood, was burnt in the middle immediately above the body, but had become extinguished before the house was opened—the flame of this wood could not have been less than six feet from the body of the woman. Immediately behind her, the ash flooring had taken fire and was burning at the time of the discovery; this fire had not progressed more than a foot from the hearth and had destroyed about four square feet of the floor. On the edge of this fire a chair was partially burnt.

The weather at the time of the incident was moist, and, for the season, warm.

Lexington, Kentucky, January 1830.

MISCELLANEOUS INTELLIGENCE.

1. *Successful Extirpation of the Uterus. By Professor Recamier.** We are happy to record another successful issue to this most formidable operation—and that in a public institution, where no colouring can be given to the facts—no veil of concealment thrown over the ultimate event.

Case. Agathy Pelagie, aged 50 years, was received into the Hotel Dieu, on the 24th July, 1829, under M. Recamier. She had begun to menstruate at the age of 12 years, and did not cease to do so till she had attained her fiftieth year. Examined on the day of her entrance into the Hospital, the following was the report. 1 mo. Slight pains were felt in the pelvis, and also in the loins—a sense of lassitude sufficiently distressing to prevent the perpendicular posture being long maintained. 2ndo. A discharge of sanious fluid, extremely fetid and sanguinolent, of about eight months' duration. 3tio. An examination shewed a loss of one of the lips of the os tincæ. The remaining one (the anterior) was thickened and ulcerated. The same alterations were perceptible in a certain portion of the vagina near the cervix uteri. This first examination did not induce any suspicion of adhesion between the parts affected and the rectum or bladder. On introducing the finger far up the rectum a tumour was found to occupy the situation of the uterus, and was concluded to be the diseased uterus itself. The various functions and the general health were in good condition. M. Recamier, who had once before extirpated the womb with success, determined on the operation in this case, and it was consented to without fear.

On the 26th July, at 7 o'clock in the morning, the patient was placed on a bed in the midst of numerous pupils and others, besides Messrs Margolain, Breschet, Palrice, and Blandin, and the operation was commenced. M. Recamier first introduced an instrument (pince de museux) with which he seized the cervix uteri as high up as possible, and then dragged it down slowly and cautiously, the body of the uterus following it, till the whole came in view from the os externum. The artificial prolapsus thus effected, the operator fixed the organ in its new situation by means of another instrument (pince erigne) confided to the hand of an assistant. He then examined the rectum, and found that the gut had not come down with the uterus. He next introduced his finger between the anterior parietes of the vagina and the corresponding face of the retracted tumour, till he came to the junction of the two, when he introduced a bistoury with his right hand, divided the cul-de-sac at the spot above-mentioned, to the extent of an inch, and then

* Clinique.

withdrew the instrument. Through this wound the operator passed his finger, and divided some loose cellular substance till he came to the reflection of the peritoneum from the bladder over the uterus. This he divided with the bistoury once more introduced. The peritoneal incision was next enlarged, first on one side and then on the other, with a probe-pointed bistoury. The operator having searched for the broad ligaments, he passed a ligature around each, to prevent hæmorrhage, and then divided them with the knife. The peritoneum passing from the rectum to the uterus was next divided, and the uterus itself dissected carefully away. The operation is said to have been conducted with remarkable sang froid,—to have occupied only twenty minutes,—and to have been succeeded by no hæmorrhage. The cervix uteri was found to be affected with open cancer; but the body of the uterus itself appears to have been very little affected. The patient bore the operation with unshaken courage, and fell asleep soon after she was put to bed. On the second day fever arose, and continued for two or three days, requiring several small bleedings. The fever ceased at the end of the fourth day, but was succeeded by a fixed pain in the right iliac region. Forty leeches were applied to this part, and the pain was reduced. The ligatures were removed on the fifth day, and on the sixth, there was tension and swelling of the abdomen. Leeches were repeated. Nothing particular occurred till the ninth day, when, on examining the vagina, a fetid, dark, and sanguinolent fluid was found issuing forth. Injections were carefully thrown up during the next few days. By the twelfth day, when the account closes, she recovered some appetite, and had taken soup repeatedly. Should any thing further be published, we shall report it. Meantime the case may be considered as a successful one, as it is not now likely that the patient will die from the effects of the operation, which appears to have been performed with great skill and dexterity.—*Medico Chirurgical Review, No. 22, for Oct.* 1829.

Note. A statement of the above case has been forwarded to us by Dr. Alban G. Smith of Danville, Ky. (now in Paris) in the conclusion of which he says, "at the end of a month she" (the subject of the preceding operation) "was perfectly well, exhibiting a living monument of Surgical skill." Eds.

2. *Statement of Cases of Syphilis, Treated in the Hospital Val de Grace.* (*Paris.*)—(We are indebted to the same gentleman, Dr. Smith, for the following statement.)

1312 patients were treated from April 16th, 1825, to July 31st, 1827. 1084 primarily affected, simple and complex cases; 228 had secondary and mercurial disease, simple and complex.

Of the 1084, 386 were treated by mercury: mean duration of treatment 47 days. 189 of these had animal and stimulating diet: mean duration of treatment 51 days. 197 vegetable diet: mean duration of treatment 42 days.

698 were treated without mercury: mean duration of treatment 28 days; of these 62 had stimulating food: mean duration of treatment 50 days. 636 with vegetable diet: mean duration 25 days. Of the 228 with secondary syphilis, 72 were treated with mercury: Mean duration 67 days. 33 had stimulating food: mean duration 82 days.

3. *Antidote to Prussic acid.—M. Dauvergne*, in a letter to *M. Gay Lussac*, dated Paris, April 25th, 1829, states, that M. Simeon, apothecary to the hospital Saint-Louis, poisoned a cat with hydrocyanic acid, by placing two drops in the corner of his eye; the animal was violently affected, and when to all appearance past recovery, a large quantity of chlorine was diffused in his throat, which very soon alleviated the symptoms, and when able to raise his head, which before he could not do, he appeared to take pleasure in smelling the chlorine, from the relief which it afforded. In an hour he rose on his feet, and in the course of two hours more, scarcely any traces of morbific symptoms remained. The experiment has been tried on various other animals, with similar results. —*Silliman's Journal Oct.* 1829; *from Ann. de Chim. Mars*, 1829.

4. *Singular case of Hydrophobia.*—A case is related in the Lancet, a London Monthly Periodical, of a man having died with all the horrors of hydrophobia, and the dog that had bitten him betraying no such symptoms. The statement is from an eminent surgeon, and is well attested by several respectable names. Soon after the man was bitten, he applied to a medical gentleman, intimating, however, his firm conviction that the dog was not mad. As the dog was evidently sick, the surgeon took the precaution of having him placed in a secure situation, and from day to day watched the progress of his symptoms till he died. From the most attentive observation, the surgeon could not observe the slightest appearance of *rabies;* the animal lay quiet, walked firmly, breathed easily, had no abhorrence of fluids, and caressed his master as usual. His death appeared to be accompanied with pain, and on being opened, the body presented none of those appearances which are generally produced by *hydrophobia.* In about a fortnight after the decease of the animal, the man was taken with the usual symptoms of *rabies*, and died in dreadful convulsions. The case has excited considerable interest, as it proves that hydrophobia, in animals, is not confined to one character, but assumes different shapes.

Remarks on the above cases and on the uses of the chloric preparations.

We have added the account of the last hydrophobic case, for the purpose of saying, that it might be prudent in every instance of a bite from a dog, or other animal, whether supposed to be rabid or not, to wash the wound frequently with the solution of chloride of lime. From what we now know of the powers of chlorine, it is not too much to hope for, that it may prove an antidote to every case of poison, provided it be applied in season, and before the system is fatally affected.

It appears highly probable that hydrogen, from its being the lightest and most subtle of all known ponderable bodies, may enter into the composition of such active agents as poisons. Fontana examined the poison of the viper and of other animals, but we know not whether, as in prussic acid, hydrogen is the active principle. In the present No. it appears (p. 174,) that chlorine destroyed the effect of prussic acid, the most active poison known, even after it had gone far towards producing death. It is therefore credible, that it may destroy other poisons, having a similar constitution; and we can readily understand the modus operandi in such cases; for chlorine takes hydrogen from every combination, and of course destroys the peculiar character of the compound. Prussic acid itself consists of nothing but the ordinary elements of animal matter, such as are daily used for food; but in the acid they are combined in a peculiar manner, and the withdrawing of hydrogen from it, at once subverts the combination, and renders it harmless.

The practical use which we would make of the facts which we know, and of the theoretical views which we entertain, is, that chloride of lime should be kept in every family ready for instant use in the multiplied cases in which it is applicable.

It is already manufactured in this country in large quantities, and we observe with great satisfaction, that it is hereafter to form a part of the stores of our ships of war. This will greatly encourage the manufacture, and the time is not distant, when the price will be much less than at present.—*Silliman's Journal, Oct.* 1829.

5. *Utility of continued or perpetual vesication in the prevention of habitual abortion.*—In a paper on this subject read before the Medical Society of the city and county of New York, Dr. Lewis Belden details the reasons which induced him to adopt the practice, and gives the following case.

"Mrs. W., a very worthy and respectable lady of this city, of temperate and regular habits, but of delicate constitution and enfeebled health, having suffered severely from three successive abortions, and being threatened with a fourth, as was evident from the premonitory symptoms, and the common and generally approved routine of practice, hitherto faithfully pursued, having failed, it was thought advisable to make trial of the irritating effects of continued or perpetual blistering. Accordingly, a blister, somewhat less than the size of a half dollar, was applied to the leg near the patella. It was directed to be dressed with cabbage leaf to promote a free serous discharge; and when that should cease, to make use of the savin ointment, to keep up the irritation and discharge; and whenever the vesicated surface became in a measure healed, (as it had a strong tendency to do,) the blister was ordered again to be applied for an hour or two, and its reapplication did not fail to produce as free a discharge as before, if not more so.* Under this

* We would here take occasion to remark, that the benefit resulting from blistering is to be ascribed to the combined effect of the irritation produced

practice the patient progressed for some time as favourably as could be well expected. At length, however, we were sent for to visit her in haste, on account of the alarming state of the symptoms present. We soon found, on making the necessary enquiries, that the blistered surface had been neglected and suffered to dry up, from her attention at the time having been somewhat more than usually engaged with company. No time was therefore lost in advising the reapplication of the blister; and after giving other necessary directions she was left for the night. When visited again in the morning we were much pleased to find that the blister had drawn well, that the pains and other unpleasant symptoms had subsided, and that the patient was out of the imminent danger which, but a few hours before, had been threatening her. The relief indeed was so decisive, that had we previously entertained doubts of the efficacy of the counter irritating effects of vesication in the case, we could reasonably give place to them no longer. As the patient, a short time after this occurrence, was desirous of going to and tarrying awhile in the country, she was directed to keep the blister open several weeks longer; and, should any premonitory symptoms of abortion occur upon drying up the discharge, to reapply it immediately. Nothing untoward after this occurred in the case; and having gone her full time, she was safely delivered of a fine son. We have also the satisfaction of saying, that her general health has been greatly improved: and that she has since then passed through another period of pregnancy, and borne another healthy looking child, a daughter; and, that no appearances or symptoms threatening abortion have occurred during the time—the disposition or habit thereto appearing to have been completely removed, in consequence of her having been safely carried through the preceding period. It thus appears that a morbid habit was overcome, which, while it continued, rendered life itself rather a burthen than a blessing. —*N. York Med. and Phys. Journal, No.* 29, *April*, 1829.

6. *Case of Hæmorrhage after extraction of a tooth from a hereditary hæmorrhœal tendency.*—A case of this kind is inserted in the Edinburgh Med. and Surg. Journal, No. 101, from *Rust's Magazine*, in which "a Jew twenty years old, thin, cachetic, pale featured, and of delicate complexion, was attacked with obstinate hæmorrhage after the extraction of the first grinding tooth of the right side of the lower jaw." He fell under the care of *Dr. Steinmetz* who applied a great variety of styptics to the part; but notwithstanding the hæmorrhage continued alarming for eleven days, before it was arrested.

The hæmorrhœal tendency in this instance was derived from the

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and the free discharge promoted by continuing the blistered surface unhealed. The latter effect, however, appears to be but little taken into account; as the dressings usually employed are calculated speedily to heal up the vesicated surface. We believe this to be a capital error in the prevailing medical practice; for unless the disease is fast giving way, the reason for continuing the irritation and its consequent discharge remains unchanged.

grandfather. Several times in consequence of a prick with a pin his grandfather was brought almost to the point of death by hæmorrhage. At last he was attacked with pulmonary inflammation after exposure to cold and fatigue. During this attack he had bloody sputa, though not to any great extent. But on the sixth or seventh day he was attacked with profuse epistaxis, which nothing could check, and which consequently proved fatal The father had in his youth been likewise several times reduced to a state of extreme exhaustion through hæmorrhage produced by trifling injuries, and would have perished but for prompt medical assistance. At the age of sixty-five he had been free from the disease for ten years. The son, Dr. Steinmetz's patient, had been several times before seized with obstinate hæmorrhage from trifling causes. Three sisters were entirely free from the hereditary peculiarity; but they imparted it to their children. One of the children was, at the request of the mother, circumcised in presence of the relater, who had extreme difficulty in checking the flow of blood.

These facts confirm the statements of former authors,—that the hæmorrhœal diathesis descends only to the male branches of a family,—that it may be communicated to them through females without these females having it themselves,—and that it ceases in old age.

OBITUARY NOTICES.

Died at the residence of Col. Thomas G. Percy, near Huntsville, Alabama, on the 12th of January 1830, Samuel Brown M. D. former professor of the Theory and Practice of Medicine in Transylvania University.

At a meeting of the Kappa Lambda Society of Hippocrates, called for the purpose, the first vice President in the chair, the following preamble and resolutions were adopted.

Whereas this Society is informed of the death of Dr. Samuel Brown, of Huntsville Alabama; whereby the community at large has lost one of its greatest benefactors, medical science one of its ablest and most distinguished votaries, and this Society its venerable Founder and President;

Resolved, that we hold in high estimation the exalted talents and scientific attainments of the late Dr. Samuel Brown.

Resolved, that we sympathise with the medical Profession at large in the loss it has sustained in the death of that distinguished individual.

Resolved, that this Society especially deplore the afflicting dispensation of Providence by which it has lost the cooperation of the learning and genius of its illustrious President, in the promotion of its benevolent objects.

Resolved, that in testimony of our sorrow, we wear the usual badge of mourning for thirty days.

Resolved, that a copy of these resolutions be transmitted to the various branches of this Society in the United States.

Resolved, that these resolutions be published in the Transylvania Journal of Medicine, and also in one of the news-papers of this place.

By order of the Society,

THOMAS C. NELSON, SECY.

Lexington, Ky. Jan. 29th, 1830.

(COMMUNICATED.)

DIED in Lexington, Kentucky, on Sunday the 31st January 1830, in the twenty-second year of his age, after a protracted illness, PATRICK CRAWFORD SNODDY, of Spartanburgh District, South Carolina. At the time of his decease, he was one of the most intelligent members of the Medical Class of Transylvania University, and was on the eve of having conferred upon him the degree of M. D. as the reward of his indefatigable industry, and highly respectable medical attainments.

He has left an affectionate father and mother, as well as brothers and sisters, to lament this untimely bereavement: nor are they alone bereaved, but we, who have had our hearts warmed by the testimonies of his friendship, who witnessed the cheerfulness with which he bore sickness, and the contentment with which he endured his great bodily sufferings, sympathize sincerely, and deeply with the more immediate sufferers in this afflicting dispensation. It is not our object to eulogise, but in the expression of our elevated sentiment of his inestimable worth, to inculcate a bright example of imitation to his youthful acquaintances, assembled for the same noble attainment, and to furnish a testimony of our esteem to those more intimately allied by the ties of consanguinity, and thus, if possible, to assuage by some cordial of consolation the wound inflicted by this irreparable loss.

DIED at Thebodauxville, Louisiana, on the 16th November 1829, WILLIAM AUGUSTUS FICKLIN, M. D. aged twenty four years. Dr. Ficklin was a native of Kentucky and an eleve of Transylvania University, in the Medical Department of which he graduated, with much respectability, in the spring of 1828. During the recent prevalence of Yellow Fever in Louisiana; Dr. Ficklin was actively engaged in attending many cases of the most malignant character: and it was not until after its general prevalence had ceased that he himself fell a victim to that formidable disease.

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ORIGINAL COMMUNICATIONS.

ARTICLE. I.—*On the Prussic Acid.* By JOHN H. SAVAGE, M. D. of Jefferson County, Mississippi.

THIS medicine had scarcely become generally known to the physicians of our country, when, after a few hasty and imperfect trials of its virtues, it seems to have been nearly laid aside, as a remedy whose properties are either too dangerous to justify its use in practice, or in which the expectations of its value had not been realized. Perhaps, too, incredulity may have had its weight in preventing this remedy from enjoying a larger share of the experience of our profession. New medicines from time immemorial have been ushered in with such extravagant and unmerited encomiums upon their virtues, that a general distrust is now entertained in relation to them; and a large majority of practitioners patiently await the decision which time and unbiassed experience may pass upon them. Whatever may be the cause, it seems pretty certain that our knowledge of the remedial power of the prussic acid, and of its proper application, has advanced but little since our first acquaintance with it.

Among those who gave it an early trial, there was some discrepancy of opinion concerning its curative effects in certain diseases, while no inquiry was attempted as regarded the *states* of disease or the *conditions of the system* to which it was best adapted. With a hope therefore of adding something to the information which we possess on this subject, and to confirm such information, as, at present, may be considered unsettled and questionable, I have communicated some remarks upon this medicine, which have been suggested by my experience with it.

Diseases of irritation seem to be the appropriate sphere for the remedial agency of this medicine—like opium, it is better calculated as a palliative than to act a leading and radical part in the treatment of a well established and important disease; to suspend morbid action rather than to subvert and remove it. But in many cases it possesses a decided superiority over the most unexceptionable form in which opium can be administered. Every physician has frequently observed, in patients recovering from fever, a restlessness of a very harassing character, where the various functions of the system are regularly performed, but where there is too much debility to admit of exercise by day, and so much excitability existing that the patient is unable to enjoy repose at night. In such a condition, opium, in any of its forms, is rendered an undesirable remedy by its constipating effects upon the bowels, while the prussic acid, free from any objections of this kind, is fully capable of answering the purpose of allaying inordinate excitability, and permitting the recuperative energies of the system to complete the process of convalescence. There are few diseases of simple and unadulterated irritation, and of course but few in the treatment of which this medicine can be exclusively relied upon. Most of the diseases of irritation are, by the laws of physiology, associated as a consequence with morbid action of a higher and more important grade, in which this remedy can only be used as an auxiliary. I have before remarked its power of suspending morbid action, or of allaying preternatural excitement for a time. This of itself presents a strong claim on the attention of physicians, for, independent of other advantages, in diseases

of increased vascular action, arising from nervous irritation, to allay the one is to cure the other. Some kinds of ophthalmia are examples of disease in which the prussic acid may be thus beneficially used. But the diseases in which pain and an increased mobility of the nervous system are prominent symptoms, are numerous, and open a wide field for the successful administration of this remedy.

In *chronic inflammations of the liver*, which are often to be met with in this part of the country, and too often occasioned by the injudicious and premature use of quinine and other tonics in our fevers, I have used the prussic acid with good effect. The patients have generally a sallow and emaciated appearance, a frequent and sometimes a hard pulse, pain in the side and shoulder, with a dry cough. They complain of an unusual liability to take cold upon the slightest exposure. In a majority of these cases, especially in recent ones, and in vigorous habits, it will be necessary to abstract blood; while in neglected cases, in delicate constitutions, and where much inroad has been made upon the general health, it will probably be most prudent to refrain from this evacuation. This is a point, however, on which no precise direction can be offered and must be determined by the judgment of the experienced practitioner. This much, however, may be safely advanced, that where the tone of the general system will bear it, depletion will be advantageously resorted to. The next step is to relieve the inflammatory action of the organ by means of secretion. For this purpose I have generally pursued a deobstruent course of treatment, consisting of calomel and comp. ext. colocyn. and sometimes in more urgent cases calomel alone. This treatment is rendered more efficacious, I have thought, by the application of a large blister over the right hypochondrium. The benign and tranquillizing influence of the prussic acid will now be experienced in allaying the cough and mitigating the general irritation which prevails. The formula which I have been in the habit of using is as follows:

℞ Acid. Pruss. gtt. viij. } m.
Aquæ rosæ ℥ij. }

Of this a teaspoonful is given three times a day. I have some-

times given two teaspoonsful after an interval of about an hour between the two. When this has been done, however, the patient has complained, at times after the second teaspoonful, of uneasiness and pain in the head and a feeling of general weakness. But this effect has subsided in the course of three or four hours without the intervention of any remedy. I may here remark that the effects of the prussic acid appear to me to be much more transient in their duration than those of opium. While the sedative influence of the latter has been thought to continue about twelve hours, that of the former subsides in less than half that time. This fact may be of some importance in regulating the mode of exhibiting it. Half a teaspoonful given every two hours would be preferable, perhaps, to the mode in which I have administered it. In this way the sedative effect of the medicine would be secured without endangering the unpleasant symptoms which arise from too large a dose.

Although there are different conditions of *pulmonary disease* in which the prussic acid might not only be admissible, but advantageous, yet the most auspicious period for its exhibition is when there is nothing more than a simple irritation of the lungs, as is manifested by a dry cough. Given at a time when copious expectoration of pus is going on, it may do much injury; for the process of secretion requires a certain degree of vital power to sustain it, and the tone of the pulmonary capillaries would be so far weakened by the effects of this medicine, that secretion would be suspended. Two cases illustrative of this effect may be found related by Dr. Randolph—(Vide Med. Recorder, vol. 4th, page 456.) So also in congestion of the lungs, with feeble efforts towards reaction, but little or no benefit would be likely to result from the use of this medicine. On the contrary we might naturally look for an unfavourable effect, under such circumstances, from the power which the prussic acid possesses of depressing the nervous energy. It seems not improbable from its prompt and peculiar influence over pulmonary irritation, as well from dissection of bodies which have been poisoned by this acid, that it exerts a much more powerful effect over the lungs than any other viscus. (Vide *ut supra* vol. 2, p. 534) The translator of Ma-

gendie's Formulary supposes the effects of the prussic acid to be expended with most power upon the mucous membranes. Under such views it might be expected to be more injurious in pulmonary congestion than in a similar condition of any other organ. I am aware that this medicine has been recommended in *asthma;* but it is justifiable only in the spasmodic variety of this disease, and even in this the prospect of its advantge is questionable. In such cases as have fallen under my notice, I have considered it totally inadmissible. The vital energy of the system generally has seemed to me to be depressed, and the lungs almost incapable of performing their office during a severe paroxysm. The only means by which relief can be obtained is through expectoration, and this must be accomplished by emetics and stimulant expectorants. Bleeding I have found injurious by still further accumulating the load about the lungs and destroying the balance of excitement. The prussic acid under such circumstances could only aggravate the disease by lessening the power of the lungs to relieve themselves of their burden. The disease as it has occurred in my practice, has exhibited the form of congestion and has depended upon hepatic derangement. A paroxysm will be preceded for a day or two by violent coughing and sneezing, shewing great pulmonary irritation. It is in this stage alone before the paroxysm has fully set in, that the prussic acid or stramonium can be usefully employed. In the incipient stage of catarrh also, succeeded by draughts of warm snakeroot tea, it may be attended with advantage; but after febrile symptoms are developed, its utility to say the least of it, will he doubtful.

In *inflamation* (and the remark may be extended to *febrile action* generally) other remedies claim a decided superiority over the prussic acid, or at most it should be used only as an auxiliary to more appropriate remedies. Inflammation is the result of congestion arising from visceral or vascular debility. This congestion acts as an irritant to the neighbouring blood-vessels, which soon extends to the whole sanguiferous system, and an increased action commences, which continues until the vessels lose their irritability by exhaustion, or the visceral obstruction be removed. Now, if the prussic acid be administered to any considerable extent,

the irritability of the organ may be paralyzed, but the obstruction will remain. It is in this way that the sulphate of quinine operates in large doses in breaking up fever, to the ephemeral reputation of the practioner, perhaps, but to the lasting injury of the confiding patient. I am aware that Hufeland, Langrish and others have reported the successful treatment of tertian intermittents by means of this acid, or some modification of it, and that it has even been recommended, when given in small and repeated doses, as a powerful resolvent of obstructions of the spleen and other viscera consequent to fever. Dr. Ellis in his Medical Formulary, says, "the author of this work is now giving it to a patient afflicted with chronic hepatitis, of many years standing, with the most beneficial effects. The patient now takes nine drops daily, and since the dose arrived at six drops, all pain and soreness of the side have disappeared, and the whole system appears to respond to its renovating properties." But this treatment appears to me to be at variance alike with reason and analogy. No physician expects to find a medicine of deobstruent power among the narcotics and to this class the prussic acid evidently belongs. There is however a variety of intermittent fever in which this medicine, in common with opium, camphor and other medicines of this class, may be useful. It is that kind which is termed 'kept up by habit.' This is nothing more I apprehend than a continuance of morbid action from excess of irritability. By equallizing the relative state of excitability and excitement, we may cure the disease. This may be done by diminishing the former by anodynes, or increasing the latter by tonics, or by the two plans combined.

Although I should by no means think of relying upon the prussic acid *alone* in any form of fever or visceral inflammation, yet it may frequently be made a useful component part of the treatment of many febrile diseases. It will have a good effect in obtunding that extreme irritability which accompanies vitiated bilious secretion. But in congestive fever, where there is obstruction with a loss of tone in the general system, it is inadmissible for reasons before mentioned. A decoction of the leaves of the peach tree (amygdalus persica,) which contains this

acid, has long been in use as a valuable anthelmintic, and I have experienced its good effect in bilious fever when preceded by calomel. In 1822, the prussiate of Iron was brought forward by Dr. Zollickoffer of Maryland, and its efficacy as a febrifuge remedy advocated with much zeal. He used it in remittent and intermittent fevers, and considered it greatly superior in efficacy to all the other tonics. It has since found other friends in this country and in Europe. How far the curative virtue of this remedy may depend upon the prussic acid, I am not able to say, nor does my limited experience with it confirm the unqualified commendations recently bestowed upon it.

In two or three cases of an *enteritic form of disease* I have used the prussic acid with advantage. In these cases there had been previously a morbid condition of the liver with a determination to the bowels, and in one of them some peritoneal inflammation. There now existed a costive state of the bowels, pain and sickness, whenever a dejection was obtained, and great constitutional irritation. Bleeding, mercurial cathartics, and ptyalism had been resorted to, and the disease for which this course had been pursued, seemed to be removed. The patients were able to sit up and walk about, but they yet laboured under the remains of disease, and suffered with the symptoms just enumerated. In one of these cases, the decoction of the peach leaf was used which allayed irritation and kept the bowels free. Five grains of the pil. hydrarg. were given every night. In another case the prussic acid solution was administered with an occasional dose of castor-oil. I have met with some cases of chronic dysentery occurring late in autumn and the early part of winter, when the original course of the disease, (a depraved state of the biliary organs) was removed, but where the disease appeared to be kept up by the variableness of the weather. I had but little or no experience with the prussic acid at that time and it was not used; but should such cases again occur, I should resort to it with much confidence of deriving benefit from it.

I used this medicine not long since in the case of a lady who had met with several abortions at an advanced period of gestation. There was some morbid condition of the liver of long standing,

and a great enlargement of the spleen which had subsisted for many years. When gestation arrived at a certain period, the draft on the powers of the system for fœtal support became too expensive for its impaired tone, and abortion consequently took place. Fever was always previously induced, however, as a consequence upon debility which followed the necessary abstraction of excitement from the general system. Bleeding, mercurial cathartics, and ptyalism were resorted to, and then mild laxatives during the day, with the black-drop at night. But fever went on and the tone of the system was continually on the decline. Tonics were inadmissible, for in such a state of visceral disease I could expect nothing but an aggravation of every evil from their use. Wine and other cordials however were used. Diarrhœa and anasarca alternated with each other. I now used the prussic acid, not from a credulous belief in its wonder-working power, but simply to allay pain and a general irritation and restlessness. These symptoms were not entirely removed but they were greatly ameliorated. I thought it had a good effect; sometimes diuretics were administered, and at other times nothing but the prussic acid together with wine and light nourishment. The powers of the system were now so much on the wane that it became an object of concern to accomplish what before I had been most anxious to avoid. Calomel was given again, and as soon as it manifested its specific effects, there was another rallying of the vital forces and abortion soon took place. The lady after a confinement of more than two months, recovered. There are several points of interest in this case, but as they are irrelevant to the subject of this paper I must pass them over.

I was called to see another lady about seven months advanced in pregnancy who was labouring under fever, pain in the head, flushed face and foul tongue. She was bled and mercurial cathartics given and she got better. Some days after, however, her bowels being somewhat confined, she took without advice some epsom salts, which was followed by hypercatharsis, and brought away copious watery evacuations. She was now threatened with abortion, and took a few drops of laudanum which checked the operation of the salts. The pain continuing next

day, I was called to see her. She had but little or no fever, and I found nothing more necessary than to allay irritation and permit the system to recruit its energies. Nothing accordingly was administered but the usual prussic acid solution and she soon recovered.

The functions of the *uterus* affect the health of the female in a very important manner. When morbidly affected, the system languishes in apathy or is corroded by constant irritation. Even in that languor which attends a retention of the menses, and which proceeds for the most part from constitutional debility, an increased nervous mobility is generally perceptible. Hence in the treatment of many chronic and sub-acute uterine affections, the prussic acid will be a useful remedy to remove the nervous irritation which accompanies them, while other remedies adapted to the nature of the disease will be required to restore the uterus to a healthy state. I have used it accordingly with success in *dysmenorrhœa.* It should be commenced several days before the menstrual period arrives and be continued through it. It is decidedly preferable to opium, which is frequently required to be given in such large doses as to derange the feelings of the patient for several days.

In two cases of *orchitis* which followed in the wake of gonorrhœa, I have used the prussic acid. In this disease, it is my practice to bleed largely, give heavy doses of calomel, and instead of the various cold applications frequently recommended, to make use of fomentations and poultices. Such was my treatment in these cases: after the inflammatory action however, is chiefly overcome, the patients not unfrequently complain of a return of pain at night. It is at this period of the disease that I have used this medicine, and it has succeeded in both instances in relieving these nocturnal paroxysms. In one of these cases there was a painful *ardor urinæ* which was thus entirely relieved. I have not employed it in many cases of recent gonorrhœa but I have no doubt it might be successfully used in relieving some of the painful symptoms which belong to it.

I have also employed it in some obstinate cutaneous diseases, and the patients have thought they derived benefit from

it, but in these it can seldom afford more than temporary relief.

I have thus offered such remarks on the use of this medicine, as have occurred to me from my experience with it. On the whole, there can be no doubt that it possesses valuable properties, capable of fulfilling important indications in the treatment of a great variety of diseases. He who employs it without discrimination and without proper restrictions, will often be disappointed, and experience defeat where he looked for success; while he who is directed in its administration by correct pathological and therapeutic views, will find it an important addition to his means of removing disease.

December, 1829.

ART. II.—*An Essay on Congestive Bilious Fever.* By ELIJAH COONS, M. D. of Tuscumbia, Alabama.

PERPETUAL fluctuation in medical nomenclature may justly be assigned as the cause of the indefinite ideas attached by some physicians to the word congestion. It is derived from the latin word *congero*, to accumulate or collect; and has been employed as a synonyme with plethora. I understand it, however, to mean a morbid distention of the blood-vessels, from a partial stagnation of their contained fluid, in some particular part of the body: plethora is a simple fulness of the vessels without morbid distention.

Under this view of the subject we may consider every case of bilious fever to be congestive: it is those cases, however, in which reaction is but imperfectly, if at all developed, (or a preternatural coldness of the surface may exist) with a feeble and tremulous pulse, shrinking of the features, shrivelling of the skin, præcordial oppression, suspension or perturbation of the secretions, &c. of which I design speaking at present. And it might not be superfluous to remark, also, that cutaneous eruptions, resembling those of scarlatina, sometimes appear in connexion with a part or all the symptoms above. I mention this fact, because

I have known physicians to be led astray by this symptom, and the patient consequently subjected to the most intense suffering from improper treatment. Cuticular eruptions also occur in intermittents: they are an evidence of irregular distribution of the vital fluid in the system.

In the outset of autumnal fever, the action of the heart and arteries seems evidently weakened, and consequently unable to propel the blood with natural force to the surface and throughout the secretory organs; and from the diminished excitability of the system, there is a recession of the blood from the cutaneous capillaries to the interior. Hence local accumulations of blood must occur in some part of the body, and, from the peculiar structure of the large venous trunks, they necessarily take place in the portal circle, where, as Dr. Johnson very justly remarks, the blood is, at all times, languid in its current, there being a slight *vis a tergo*, and but little muscular propulsion. This state of the circulation is manifested by paleness and shrinking of the features and surface of the body, small inelastic pulse &c. If from any cause, the heart fails to recover its energy, a continuance of those symptoms which characterize congestive fever is the consequence. That weakened action of the heart produces accumulation of blood in the venæ cavæ and their branches, is now I believe, conceded by all enlightened pathologists.

The organs most apt to become affected from accumulations of blood in the portal system, on account of the laxity of their texture, or peculiarity of their circulation, are the liver, the spleen, and the brain. As the liver is in this climate peculiarly liable to sustain the onus of the disease, I shall confine my remarks principally to that organ. Indeed, we find in almost every case of fever, that the secretion of this viscus is perturbed or vitiated, indicating that it has become the centre of irritation.

The influence of hot climates, in deranging both the function and the structure of this organ, has long been understood by the profession. During the summer months, from the long continuance of the warm weather, heat acting as a stimulant, the capillaries of the liver become torpid, and portal accumulations ensue,

with deficient secretion of bile, costiveness, anorexia, debility, &c. If this condition of the system be not removed by a change in the weather, or the aid of medicine, those accumulations are apt to increase, and hepatitis will be the consequence; or, if the individual be exposed to the influence of malaria, fever will in all probability be the result.

Each organ of the human body has a certain portion of irritability peculiar to it: if an undue quantity of stimulus be applied to it for a length of time, although it produces increased action at first, yet it loses during action, part of this principle, and becomes deficient or exhausted. Thus it is with the skin in hot climates, the small vessels of which become torpid from the continued application of heat, and from the dependance of functions existing between this organ and the liver, it is peculiarly, liable to partake of the torpor that pervades the skin, which is, as I have before remarked, generation of hepatic congestion, perverted secretion of bile, &c. "When light, the natural stimulus of the retina, impresses it in a certain degree, the organs being perfect, vision is perfect. But, let this natural stimulus be increased, and, consequently, the actions resulting from its impression, in the organic apparatus of vision, be augmented, vision becomes imperfect, the function or offices of the visual organs are impaired, and, if the stimulation be carried to a high degree of intensity, as in gazing at the sun in the meridian, the imperfection of the sense is at its height. The continuance of this state of excitement, for a length of time, would be productive of permanent structural disorder, and an irreparable organic lesion would ensue, to the entire destruction of the sense".* And again, in speaking of congestive irritation, the same writer says: "This form, in its acute character, occasions a rapid determination of blood to the organ in which it occurs. It affects chiefly the highly vascular organs, as the lungs, brain, liver, &c. by which they are engorged, their functions embarrassed or totally suspended, and the equilibrium of the circulation destroyed, by the quantity of blood detained in them, and withdrawn from the general mass of circulation. In this condi-

* See Dr. Jackson on irritation.—Philadelphia Journal, 1826-7.

tion of those organs, the patient is threatened with immediate suffocation, or overwhelmed with apoplexy, or suffers extreme pain in the hypochondria, and general distress of the system."

It is a law of the animal economy, that, if irritability be increased in one or two organs, it is consequently diminished in a corresponding degree in other parts of the body. The irritability of the skin becomes, in some degree, exhausted from excessive heat, or its long continuance; and by the withdrawal of that stimulus, which caused the consumption of this principle, a restoration of it naturally ensues. Hence the prejudicial effects of wearing flannel next to the surface in hot weather. And hence the application of cold, which is the diminution of the surplus of heat, invigorates the frame. This should however be gradually increased, or continued but a short time for a very obvious reason. The effect of intense cold is a contraction of the cutaneous capillaries and weakened action of the heart, which is necessarily and inevitably followed by increased accumulations of blood about the right ventricle of that organ, and visceral engorgements consequently ensue. Here we see the beauty and wisdom of Providence displayed, on the one hand, in causing autumn to precede winter; and, on the other, the extreme danger of long continued *cold* bathing. The utility of the cold bath, notwithstanding, in hot climates, when properly used, cannot be questioned: nor can it have escaped the notice of even the most superficial observer, that the approach of winter is followed by almost universal health. The bath removes the torpor of the capillaries of the skin, and thus sustains the normal circulation of that organ: this salutary impression, on the surface, is imparted to the central organs of the system, and thus are portal accumulations of blood obviated. This is the effect of the shower bath, or dashing cold water on the body. But if an individual be immersed an unusually long time in cold water, the action of the heart will be found feeble and languid, and the temperature nearly extinguished, rendering the production of reaction exceedingly difficult.

The immense importance and almost unlimited influence of the hepatic apparatus over the animal economy is well under-

stood by all the enlightened members of the profession. Nor is its range of sympathies at all remarkable when we recollect, that it is supplied with nerves from the pneumo-gastric, or eighth pair, the phrenic, and the great sympathetic. It is extremely vascular, and is formed principally by the ramifications of three kinds of blood-vessels, the vena cava, the hepatic artery, and the vena hepatica. Now whenever irritation is, from any cause established in this viscus, there is a determination of blood to it; and, if the irritation be intense, the fluids are not confined to its vessels, but extend into its interstitial spaces. Whenever irritation is excited in any part of the body, there is a rapid determination of the fluids to the organ in which it occurs. "*Ubi irritatio, ibi fluxus*, is an aphorism derived from the sage of Cos, which has been verified by all subsequent observation, and is confirmed by every day's experience." In this congested state of the liver, there is a complete remora of the circulation, and its secretion is universally found to be abnormal, or suspended, varying for the most part upon the degree of engorgement. The weakened action of the heart, at this time, also favours an increase of the congestion of this organ,—the ingress of blood *exceeds* its egress—which may eventuate in inflammation, or disorganization of its structure.

When congestion of the liver occurs, which, in this climate, is frequently the case in autumnal fever, we have one of the most formidable diseases to combat, that is incident to it. Its most prominent features are a feeble, labouring and constricted pulse; tongue dry and coated with a yellowish fur; præcordial oppression; gastric irritability; the adnata tinged of a yellow colour, and the skin sometimes assumes a chlorotic appearance; swelling, tension and pain of the right hypochondrium; fulness and tension of the abdominal parietes; suspended secretion of bile, evidenced by the argillaceous or ash-coloured appearance of the alvine evacuations; obstinate constipation; urine highly coloured and scanty, and sometimes dysury; wild delirium, watchfulness; great anxiety and peculiar restlessness, with cold extremities, &c.

If medicine be opportunely and skilfully administered, a favour-

able change generally takes place about the 7th or 9th or, at the farthest, the 15th day, which is indicated by an abatement of all the symptoms. But, on the contrary, if the character of the disease be severe, or if it be improperly treated, the stage of collapse sometimes supervenes, which may always be considered "a change pregnant with the utmost danger to the patient."

The precursory symptoms of death are, a red, polished and pointed tongue; intestinal hæmorrhages; thin black discharges from the bowels, with griping; jactitation; low delirium, dilated pupil, and tinnitus aurium; hiccough; hypochondriac and epigastric oppression and soreness; a feeble, thready and variable pulse; cold cadaverous sweat; and coma.

Treatment.—What are the indications of cure? The answer is plain and cannot be misunderstood,—remove the congestion and establish a healthy secretory action in the system. Evacuations, then, both from the sanguiferous system and alimentary canal are *absolutely* necessary: but our chief reliance must be placed in *purgation.* And, in the administration of purgatives, I will say once for all, *that the dose should be graduated to the violence or obstinacy of the incursion, and continued till the effect desired is attained; viz. the free discharge of consistent bilious matter.* The procurement of those discharges is a proof that the medicine has performed its office, and should therefore be continued till the disease is subdued. A certain class of physicians, however, consider cathartics beneficial in proportion to the quantity of the discharges they produce, without any further regard to their character, and for this reason give a decided preference to hydragogues. The educated practitioner, on the other hand, cautiously avoids their use, knowing that the relief of his patient does not depend so much on free purging, as the nature of the discharge. Indeed, it is a notorious fact, and one that must have arrested the attention of every member of the profession, that the symptoms of portal congestion may be increased by the operation of a dose of sulph. magnes., or a combination of jalap and cream of tartar: ample experience proves, that they may be as certainly removed by a preparation of calomel, aloes, scammony and rhubarb, in equal parts.

But am I met at the very threshold and told that this combination, as well as mercurial cathartics, will produce a morbid action in the liver, and consequently the alvine evacuations are of a dark green or blackish colour; that its secretion is suspended, in the first instance, from too much excitement, and as calomel increases its action, it is therefore injurious? Now, these are the premises which some physicians assume, with their deductions: they therefore demand a passing notice.

As to the first position I would say, that those discharges are the effects of the proper operation of the medicine upon the biliary system, and although the action produced by it be different from that of health, yet it modifies or changes the "morbid action," and thus, by breaking up the concatenation of the disease, a restorative normal function ensues: and moreover, universal experience proves, that the secretions become healthy under the influence of the protochloride of mercury.

Secondly. I grant that the biliary secretion has become vitiated or suspended from *inordinate* action of the liver. The reason of this is obvious. It is a law of the animal economy, as I have already attempted to show, that, if stimuli be applied to any organ, and kept up a length of time, it produces increased action at first, but it is as universally succeeded by a corresponding degree of torpor. Now, this is precisely the case with the hepatic apparatus at this time, and we may purge and purge as much as we please, but unless the biliary ducts be emulged of their viscid contents, the patient cannot be relieved. Indeed, we might as well expect to prevent suppuration, in a case of inflammation of the female mamma, without disgorging its tubuli lactiferi of their stagnant milk. And the practitioner, who relies on saline cathartics in these cases, will be most egregiously disappointed: they act principally upon the mucous glands of the alimentary canal, and may produce large liquid discharges by stool, without affording any relief to the patient, because the *pori biliarii* and excretory ducts remain gorged with viscid and depraved secretions. If this condition of the biliary system be not removed, suppuration or disorganization will be the result. Its secretion must therefore be reinstated as soon as

possible; and this must be effected by such remedies as act at once on the hepatic organs and whole tract of the intestinal tube. For this purpose, either of the following formulæ may be used: they have peculiar influence in exciting both the secretory and excretory vessels of the liver:

℞ Calomel,
Aloes,
Scammony, *aa* ʒj.
Tart. Ant. gr. j.
} Make xxx pills.

Or,

℞ Calomel,
Ext. Colocynth.
Jalap, *aa* ʒj.
Pulv. Camph. gr. x.
Ant. Tart. gr. j.
} Make xxx pills.

Six of these pills may be given and repeated every six hours till the effect desired is obtained; viz. the free evacuation of consistent bilious matter. If those discharges can be procured at an early stage of the disease, and can be maintained, the patient may *always* be considered safe.

It is contended, however, by that class of physicians styled *medicins physiologistes*, or the followers of Broussais, that cholagogue remedies should be proscribed in that form of autumnal fever now under consideration, for the same reason that saline purgatives should be, in gastro-enteric irritation,—because they are irritants, and irritants are inadmissible in irritations. What perfect sciolism!! They seem entirely unaware of the fact, that therapeutic agents possess the power of changing or diminishing diseased action, or of breaking up morbific associations upon the principle of substitution; and that the quantity of fluid evacuated, in gastro-enteric irritation, by purgatives, counteracts the stimulus of the medicine, and thus acts beneficially in reducing the intensity of the irritation: revulsive measures are therefore relied upon to the exclusion of almost every thing else. Some of those physicians indeed appear to have never once thought, that febrile affections may be cut short by a judicious employment of therapeutical means. The Pinelian or Broussaian mode of treating fever may succeed in France, or in

some parts of the United States, for aught I know, but the practitioner who adopts it in the *bilious* fever of this country, will consign no small number of his patients to an untimely grave. And he who relies on the Peruvian bark in the treatment of this disease may expect to meet with a like fate. Either mode is pregnant with absurdity. The experience of every age has been decidedly opposed to the employment of the bark in the management of the first stages of bilious fever: yet it was impudently averred, in my presence, on a certain occasion recently, by one whose experience ought to have taught him differently, that, if he were "compelled to take any one article of the materia medica to the exclusion of all others in the treatment of *bilious* fever, it would be the bark." This declaration seems to me, at least, highly savoured with empiricism. Even, in the early stages of intermittents, the bark will probably be ejected, and consequently of no use—or, if retained, not unfrequently proves detrimental by converting them into remittents, or even into the continued type. But after sufficient depletion it will be found a valuable remedy. In the North, where there is a prevailing phlogistic tendency, venesection is an essential preliminary to the use of this medicine; whilst in the South, where a bilious diathesis prevails, chologogue remedies are necessary to prepare the system for its reception.

If there be a tendency to inflammatory action in the system, "the bark can seldom be employed with any decided advantage, unless it be preceded by venesection, and other evacuants. By one good bleeding and cathartic, in instances of this kind, the fever will commonly assume its genuine character; the rigors will be strong; the perspiration which concludes the hot stage, copious and general, and the intermission perfect. The bark will now act beneficially, however ineffectually it may have been previously employed," &c.* And Dr. Gregory says, in cases of "plethoric and inflammatory systems, and when the liver and spleen are affected, active and direct depletion is advisable." "We (he continues) have the assurance of Pringle

* Eberle's Therapeutics, p. 189, vol. 1.

and Cleghorn,"* that in warm climates and seasons, it (blood-letting) is a safe and proper practice, rendering the intermission or remission more complete, taking off the inflammatory diathesis which counteracts the beneficial effects of bark, and removing those pleuritic and rheumatic affections, and those symptoms of congestion in the brain, which are sometimes complicated with ague. The blood drawn in the hot fit of an ague frequently exhibits the buffy coat." Professor Cooke, of Transylvania University, is also a strong advocate for evacuants in intermittents. Without previous evacuations, "the fever becomes obstinate and the bark dangerous from its stimulating quality.† Sometimes it is necessary to repeat the venesection several times. Dr. Johnson advises venesection till the head and precordia are relieved without regard to quantity, taken particularly in the height of the fit, and drawn into the centre and not on the side of the basin, which should be smooth, otherwise the inflammatory buff does not appear."‡ And again: "The early use of the bark in these cases (of an inflammatory diathesis) produces stricture of the chest, an increase of fever, black stools; and if continued, cachexia, suppuration of the liver, dropsy and even melancholy and its consequences: also, stupor, diarrhœa, spasms and death."§ In these cases purges and phlebotomy succeed completely in affording relief.

"With respect (says Dr. Potter of Baltimore)‖ to the treatment of intermittents generally, we must remark, that the great defect in the treatment consists principally in the early and persevering use of the tonics and stimulants. * * * * The great indication of cure is to emulge the vessels of the liver, to cause it to take on a secretion healthful in quality and quantity. This is to be effected by mercurial cathartics, and by blood-letting, if the action of the heart be strong, and by tonics after these evacuations, in some cases, *but they are seldom required, if*

* Pringle on the Diseases of the Army, p. 200. Cleghorn on the Diseases of Minorca, p. 197.

† See Senac, p. 170–1, and R. Jackson on fevers of S. Carolina.

‡ Calhoun's Annotation to Gregory, p. 125, vol. 1.

§ Calhoun's Annotation to Gregory, p. 133.

‖ See Gregory's Practice by Potter and Calhoun, p. 137.

the depleting means have been properly extended." My experience accords precisely with the foregoing remarks by Dr. Potter. Stimulating cathartics frequently remove the disease, in this climate, without the employment of the bark or its salts.

There are cases, certainly, in which, after internal congestion has been removed by evacuants, the bark may be beneficially employed for the purpose of imparting tone to the affected organ and maintaining the equilibrium of action between the capillaries and larger trunks of the sanguiferous system, thus preventing a reaccumulation of blood in the venæ cavæ, and consequently warding off the chill. "If," says Dr. Bell of Philadelphia, "the period following the paroxysm be not one of complete apyrexia, and there remains some heat and tenderness in the epigastric region, with a furred tongue and red on its borders, we shall for the most part only increase the general discomfort of the patient by administering the bark and other tonics, wine and rich food, even though, by this treatment, we prevent the coming on of the next paroxysm." And again: "The smallness and fulness of the pulse should not deter us from the use of evacuants when the stomach is distressed, the head pained, the hypochondria tender and tumid. * * * * In all protracted cases," says he, "of the disease, bleeding is of absolute necessity, if we desire to remove the sustaining cause; viz. either chronic inflammation of the liver or spleen, or of the gastro-intestinal mucous membrane, or sometimes all conjoined. The supervening of anasarca, abdominal dropsy, chronic bronchitis, or pleurisy, will demand similar treatment: and until the local malady be removed and the stomach clear of irritation, of which we shall find the evidence in a tongue moist and divested of preternatural redness, we cannot hope for any good from the bark and its kindred preparations. On the contrary, painful and protracted disease variously masked will too often be the consequence of their exhibition, when these rules are disregarded."*

Having shown clearly, I flatter myself, that the bark is not

* North Amer. Med. & Surg. Jour., for October, 1829.

admissible in the early stages of intermittents, and indeed but seldom *actually* required at any period of the disease, we now come to the consideration of its use in bilious fever. Dr. Clarke asserts, that "as soon as the intestinal tubes have been thoroughly cleansed, the cure must *entirely* depend upon giving the Peruvian Bark, in as large doses as the patient's stomach will bear, without paying *any regard to the remission, or exacerbations of the fever!!* But behold the shocking consequences of this practice. Dr. Johnson, with the candour truly characteristic of an extraordinary mind and magnanimity of soul; tells us, that after prescribing an emetic, in a case of bilious fever, which "discharged a quantity of ill-conditioned bile, both upwards and downwards: soon after which, a perspiration broke out, the febrile symptoms subsided, and a remission almost amounting to an intermission, followed. I now, with an air of confidence, began to 'throw in' the bark; quite sanguine in my expectations of soon checking this formidable disease. But alas! my triumph was of very short duration; for in a few hours the fever returned with increased violence, and attended with such obstinate vomiting, that although I tried to push on the bark through the paroxysm, by the aid of opium, effervescing draughts, &c. it was all fruitless; for every dose was rejected the moment it was swallowed, and I was forced to abandon the only means by which I had hoped to curb the fury of the disease; * * * and my patient died, on the third day of his illness, perfectly yellow—vomiting to the last, a dark fluid resembling vitiated bile," &c. The post mortem examination of this case led our author to a very different mode of practice in the treatment of this distressing malady. Evacuants were relied upon: it is needless to say "with much better success."

"On laying open the abdomen (continues the Dr.) I was surprised to find the liver so gorged, as it were, with blood, that it actually fell to pieces on handling it. Indeed, it appeared as if the greater number of the vessels had been broken down, and almost the whole of the interior structure converted into a mass of extravasation. The gall bladder contained a small quantity of bile, in colour and consistence resembling tar,

and the ductus communis choledocus was so thickened in its coats, and contracted in its diameter, that a probe could be scarcely passed into it. Marks of incipient inflammation were visible in some parts of the small intestines, and the internal surface of the stomach exhibited similar appearances. * * * * It is pretty clear that it would have required some ingenuity to devise a more injudicious mode of treatment, than that which I pursued. But it taught me an important lesson, it opened my eyes to my own folly, and—pace tantorum virorum! to the over-sight of my teachers."*

Other authority might be cited, were it necessary, to establish the fact, that the bark is not only improper but highly pernicious in the treatment of the early stages of bilious fever: indeed, it is not necessary in any stage of the disease, if the case be properly managed in the outset. It is true, when the patient has been incorrectly treated, and is worn down by the long continuance of disease, and proper evacuations have been premised, the bark or the sulphate of quinine may be employed with advantage.

I repeat, that the Broussaian plan of treating fever cannot, possibly, from the nature of the disease be successful in the bilious fever of this country. It is true, that ptisans, *lavemens*, poultices, &c. may be employed advantageously as adjuvants, but they should be used for no other purpose. The force and correctness of the following remarks from the pen of Dr. Miner, cannot fail to be recognized by every judicious and observant southern practitioner, that "the physician who only employs a palliative treatment, resembles the temporising, nerveless politician who endeavours to repel foreign aggression, by bribing the enemy or paying tribute; but he who combats disease by making an early and powerful impression, is the efficient general, who transfers the seat of war into the enemy's country."

In some cases of congestive fever, there is great insensibility of the bowels to the action of cathartic medicine, and it becomes absolutely necessary to increase the dose at least four fold.

* See Johnson on Tropical Climates. vol. 1, p. 74-5.

Professor Cooke, relates a case in which he gave six pills of aloes, scammony and rhubarb, 10grs. each, every hour, for three days before they operated.* In such cases, I have generally given aloes, calomel and scammony, each ℈ i. with 1 or 2 gtt. of croton oil and ½gr. tart. ant. made into six pills, every four or six hours, till they produced the effect desired. Catharsis *must* be effected or the patient will inevitably die: we must be governed by the effect produced rather than the quantity of medicine given.

I had a patient during the present fall, who had been ill about a week, of congestive bilious fever, and had gastric irritability, frequent retching, obstinate torpidity of the bowels, jactitation, epigastric oppression and tenderness, tension and fulness of the abdominal parietes, strangury, feeble, indistinct and fluttering pulse, &c. He took 100 grs. of calomel at one dose which had a very happy effect in composing the stomach—I have frequently seen it, in ℈ij. doses, compose the patient completely in a few hours;—eight hours afterwards, I gave him a preparation of calomel, scammony and aloes, each ℈j. and 4 grs. of pulv. camphor, which was repeated every four hours, till he took five portions of this medicine, before it operated. The discharge was of a black consistent character: it afforded considerable relief. Chologogue remedies, in exceedingly large doses, were necessary for several days to sustain the discharge from the bowels. As soon as the biliary system was unlocked, the flow of urine was remarkably great, which continued as long as the medicine was used. He recovered. My friend, Dr. W. H. Wharton, who by the way, is an ornament to the profession, saw this case in conjunction with myself. I could multiply cases of this description, were it requisite, which imperiously demanded a bold, energetic and decisive mode of practice. Under this plan of treatment the disease will almost invariably yield.

In deeply congestive cases, where the abdominal viscera are paralized from the violence of the incursion, stimulants may be united or alternated with chologogues, but they are seldom if ever necessary after the hepatic ducts have been evacuated of

* See No. III, Transylvania Journal, &c.

their viscid and strangely vitiated contents. Dr. Armstrong says. "in the worst cases, (of congestive typhus) the moderate exhibition of diffusible stimulants is sometimes really necessary; not only to support the vis vitæ immediately under depletion, but also to contribute, after its employment, to rouse the heart and arteries, that the natural balance of the circulation may be finally restored. Indeed early depletion itself is to be considered as a stimulus, since it diminishes or removes those congestions which oppress the vital functions, and thereby it tends to produce an uniform and general excitement."* In these cases the vitality of the central organs appear to be nearly exhausted, and therefore stimuli may be beneficially employed. The *hot* bath also conduces to the fulfilment of the same end; viz. the removal of internal congestion. We must, however, chiefly depend on such remedies as act on the biliary organs: if a copious secretion of bile can be effected, and kept up a sufficient length of time, the congestion of the portal system will be removed, and the patient invariably relieved. 'There is, as I have already stated,† no danger of prostrating the patient by free purgation, but on the contrary his strength and powers of life are manifestly augmented by it.' This is the case when we can obtain discharges of fœculent bilious matter. If the stools brought away be not of this character, it is common to find chocolate-coloured motions passed, sometimes in vast quantities, reducing the patient to a state of great weakness. The secretion of the liver must be excited; and when temporary exhaustion occurs from the operation of the medicine, which I repeat is rarely the case if the evacuations be of a consistent bilious nature, gentle stimuli may be administered to resuscitate the patient.

In the autumn of 1826, I was solicited to see a patient in a low fever, when life had been despaired of. He had been sick a fortnight. I found him speechless, but when interrogated as to his situation he would put his hand on the pit of his stomach: there was great muscular debility, tongue coated with a brown

* Armstrong on Typhus Fever, p. 119.
† See No. VII. Trans. Journal, &c.

fur, the teeth covered with black sordes, tension and soreness of the epigastrium, extremities covered with a cold clammy perspiration, pulse quick and somewhat tense, and subsultus tendinum. His father-in-law informed me, that he had taken no cathartic medicine for several days, for fear he would be prostrated by its operation. Notwithstanding the apprehensions of the old gentleman, and indeed I might add the physicians who had been attending him, of exhaustion from the operation of purgatives, I gave him 3j. of the protochloride of mercury, 5 grs. of pulv. camph. and a half gr. tart. ant. This preparation acted in the first instance as an anodyne, and in about ten hours brought away by stool, a copious discharge of black consistent offensive matter; the vital energies of the system were evidently augmented by it: he was now ordered to take 10 grs. of calomel, with 5 grs. camph. and one fourth of a gr. of tart. ant. every eight hours till his discharges became natural. The next day he was manifestly much better. Under this treatment he recovered.

During the same fall, I was sent for in haste to visit a lady in a similar situation. Her bowels were obstinately constipated, with fulness and pain in the right hypochondrium, and she seemed to be rapidly sinking into a state of complete exhaustion. She took 100 grs. calomel, 5 grs. pulv. camph. and a half gr. ant. tart. about 5 o'clock P. M. Through the night she was ordered to take under doses of tart. ant. and if the medicine she had taken did not operate by next morning to take oleum ricini. The next day at 12 o'clock, when I saw her, the medicine had acted well, producing copious evacuations of black consistent matter: she was better, and under the purgative plan of treatment recovered.

In December, 1827, I had a patient with the usual phenomena of congestive fever. He had a strong determination of blood to the head, with dilirium, double-vision, cophosis, &c. I gave him 3ij. of pills composed of equal parts of calomel, scammony, aloes and jalap: they produced a very copious discharge of a dark green appearance in fifteen hours. When I saw him the next day, his powers of life were evidently increased; but

owing to the inequality of the excitement and circulation, he was put into the warm bath; and afterwards sinapisms were applied to the abdomen and extremities: the pills were repeated. On the following day I saw him at 11 o'clock: he was better except the cerebral affection: the pills had acted well: blisters were applied to the inferior extremities: pills continued. The fourth day the superior extremities and nape of the neck were blistered: pills continued. On the 5th day he was much better; and recovered under the use of ʒj. of those pills.

At the same time, I had under my care a patient similarly affected with the one last mentioned. He complained of intense pain in the head, had dilated pupils, preternaturally red, polished and narrow tongue, pulse quick and tense. He was bled, and purged *freely* with chologogues, and lastly, blistered. His discharges were highly bilious and at first very offensive. Under the operation of these remedies he recovered.

During the present season, I had a patient with congestive bilious fever, who had eruptions on the surface, resembling scarlatina, great irritability of the stomach, obstinate costiveness, dry brown tongue, &c. He was bled, and copiously purged with chologogues. During the exacerbation, he was sponged with cold vinegar and water, and took sub-doses of tart. ant. He recovered.

In this low grade of fever, hæmorrhage of the bowels sometimes takes place. This is however seldom the case if sufficient depletion of the portal and mesenteric circles has been premised.

Hæmorrhage proceeds from the liver more frequently, I imagine, than many practitioners suppose; because, as I have already shown, in congestion of this organ, the blood is propelled into it more rapidly than it is transmitted into the general circulation; and its secretory vessels being naturally delicate in texture and larger than those of any other glands, readily become gorged with blood, and at last nature ruptures those vessels and suffers the superabundance of their fluid to pass off. The character of the blood from this viscus is different from that passing from the intestines, it being of a dark or black grumous

appearance. Mercurial cathartics are chiefly relied upon in such cases. By effecting a bilious discharge from the bowels, the hæmorrhage ceases. On this topic, Professor Cooke has made some judicious and valuable practical remarks.*

As to the prejudicial effects of serous discharges, which are uniformly produced by saline cathartics, every experienced practitioner must be convinced: they are "poured out by the capillary vessels opening upon the internal surface of the intestinal canal, and consequently proceed from the arterial system. Such discharges it is well known weaken the pulse, which is a certain indication that the action of the heart is weakened; and weakened action of the heart is inevitably followed by increased accumulation of blood in the vena cava, &c.; and consequently by an increase of the symptoms dependent upon or flowing from it."† A proclivity to those discharges is readily overcome by the employment of chologogue means.

In January 1826, I was desired to visit an old gentleman who had been ill several days. He had endeavoured to relieve himself by taking a dose of sulph. magnes. every day, the use of pediluvia, and restricting himself to chicken water or gruel; but evidently grew worse: he now had a dry brown tongue on the centre, with its borders preternaturally red, intense irritability of the stomach, epigastric oppression and tenderness, pain in the right hypochondrium, a quick, feeble and tremulous pulse, peculiar anxiety in the expression of the countenance, chocolate coloured discharges from the bowels, with coldness of the extremities, twitching of the tendons, &c.

He was ordered 30 grs. calomel and 4 grs. pulv. camphor, which allayed the irritability of the stomach, and, in six hours, increased the force and volume of the pulse, at which time he took another portion of the same: the hot bath was also resorted to, which had a very happy effect in augmenting the energy of the heart and equalizing the excitement: shortly after leaving the bath he had a large offensive discharge from the intestines of a dirty green appearance. I now left him with directions to

* See the III. No. Tran. Jour.
† See Cooke on Autumnal Diseases, No. III Tran. Jour. &c.

take ʒss. of pills composed of calomel, aloes, pulv. camph. and ant. tart. every eight hours till he was entirely relieved. Two days after, his son called to inform me that he was much better, and said the pills had brought away an immense quantity of black offensive matter, but that the discharges were then green tinged with yellow. The pills were continued a week, and he recovered.

The importance of the hepatic apparatus, and its peculiar liability to congestion in autumnal remittents have been pointed out above. I have likewise shown that saline cathartics are prejudicial in the treatment of the congesive type of fever, whilst chologogues appear consonant with the true pathology of the disease: they exalt the sensibility and contractility of the biliary organs, and produce the flow of bile, *that* stimulates the bowels to a brisker peristaltic action, whereby the blood, which had been accumulated in the portal system, is propelled to the periphery, and the action of the heart being increased, a restoration in the equilibrium of the circulation is produced. "Another salutary effect (says Dr. Johnson) is produced by the sympathetic influence (or in other words dependence of function) which the internal surface of the alimentary canal exerts on the cutaneous surface of the body," and, I will add, vice versa. Hence the utility of the warm bath and rubefacients: they promote the cutaneous circulation, and thus draw from the oppressed organs within, their superabundant blood, whilst the excitement imparted to the skin revives, by sympathy the oppressed powers of those organs.

The relief of the patient, as I have before remarked, does not depend so much on the quantity as quality of the discharges. About three or four large bilious discharges should be procured daily till the disease succumbs: as it yields, the quantity of the medicine may be diminished, or given at longer intervals: after portal congestion has been removed and the secretions have become healthy, the calomel may be omitted, or if the mouth become sore, it must for a time be discontinued.

During convalescence it will be necessary to procure one or two consistent discharges daily, with medicine, till the bowels ac-

quire their healthy action. Tonics are *injurious*, unless internal congestion be entirely resolved; and in that case, they are almost always *unnecessary*: they generally produce an artificial appetite, and cause more food to be taken than the system requires, or the digestive apparatus can assimilate.

December, 1829.

Art. III.—*An Essay on the Absorbent System and its Functions.* By James Conquest Cross, M. D. of Fayette county, Kentucky.

[continued from page 125.]

Divisions of the absorbent system. In the preceding pages we have adduced, from sources recognized by the profession as unexceptionable, evidence sufficient at least to prove, that, in the structure of the tissues of the animal body, no element is more extensive or plays a more conspicuous part than the vessels of the absorbent system. The next object of inquiry that presents itself is, the separate and detailed consideration of the three orders of vessels of which, we have already said, the absorbent system consists. The first of these orders or divisions is that which we have denominated

Lymphatic absorbo-exhalents. The vessels of which this order is composed have been usually termed by anatomists and physiologists *lymphatics*. Besides the reason we have already assigned for declining the use of a phrase familiar with the profession, it may not be unimportant to give a more ample exposition of our motives, as innovations of this kind, unless they proceed from the most substantial causes, are totally inexcusable.

Being obliged to create names for two orders of vessels, not hitherto recognized as integral parts of the absorbent system, it became necessary, from a wish to make them as comprehensible as possible with regard to the nature, functions and origin or termination of these vessels, to invent terms, which, in more than

one respect, clearly indicate that they are intended to designate vessels different from those denominated lymphatics. These terms also discover the fact, that the vessels to which they are applied, possess properties that are common to the lymphatics, and that our nomenclature may be extended to them with at least two advantages. In the first place, we derive from the phrase *lymphatic* nothing more than a vague idea of the fluid circulated, while it has no particular reference to two important functions performed by them, *i. e.* absorption and exhalation. In the second place, the term here suggested clearly developes the fact, that they constitute one of the three orders into which we have divided the absorbent system, and indicates also their nature and functions as well as the fluid circulated. These are considerations evidently of importance, and such, we feel persuaded, as will convince the reader that the innovation proposed is fully authorized.

The general character of this order of absorbents, is so thoroughly understood, that to detain the reader at this time, with tedious details, would be a work of supererogation, particularly as much that might be said under this head, would have to be repeated when speaking of the other two orders of absorbents. We shall, therefore, rest satisfied for the present, with remarking, that the lymphatic absorbo-exhalents arise from the villi of the intestines, from the capillary arteries, and terminate in almost every vein of any considerable magnitude. Of the truth of these propositions, ample evidence will be found under the two preceding heads to which we have just made allusion.

Arterial absorbo-exhalents. Although the extreme subtilty of the matter employed by Ruysch in his numerous and celebrated injections, led him into error with regard to the nature of the vessels into which it insinuated itself, he was constrained to admit, notwithstanding his ardent predilections in favour of a particular hypothesis, the existence of an order of vessels, that originated from the capillary arteries, and which were by him denominated *chylo-serous.* Upon the sclerotic coat of the eye he detected a vast multitude of excessively minute, transparent vessels that originated from the capillary arteries. These, however,

he refused to consider absorbents, because he could not discern valves in them. In one of his plates, which is intended to represent the proper membrane of the uterus, we observe an immense number of diaphanous and vermicular vessels delineated, but the author has not with sufficient precision pointed out the sources from whence they originate. The observations of Ruysch here alluded to, when viewed independently of other considerations, do not possess that accuracy, which alone can authorize the philosophical physiologist in making deductions upon which we can with safety rely. When, however, they are taken in connection with the microscopic researches in minute anatomy, that were soon afterwards prosecuted, and with a most distinguished and encouraging success, they present to the inquirer in this department of science sufficient interest not only to challenge attention, but to induce him to consider them as the sole cause which prompted to every subsequent extension with this instrument. We are therefore fully authorized to attempt to rescue the labours of the venerable Ruysch, that old octogenarian, from the unmerited oblivion into which they, together with those of his contemporaries and more immediate successors, were unjustly precipitated. No more substantial reason can be assigned for this most emphatically unscientific conduct, than because they were interwoven by some, and completely identified by others, with the fanciful and untenable speculations of the illustrious Boerhaave, the eloquent and fascinating lecturer in the university of Leyden.

To the celebrated Vieussens, the physician of Mad. de Montpensier, who was distinguished alike for the accuracy and microscopic minuteness of his anatomical researches, and the courtly elegance of his manners, are we, in an especial manner, indebted for the first full and satisfactory evidence of the existence of the class of absorbents now under consideration. A circumstance entirely fortuitous in its character, originally opened the way to the subsequent important and truly splendid discoveries of this most excellent anatomist. He was engaged in an examination of the gravid uterus with the microscope, and he, for the first time, obtained an imperfect view of the

arterial absorbo-exhalents. The existence of these vessels being a suggestion entirely new to him, he determined to prosecute the inquiry, and to develope if possible their nature and extent.

For this purpose, he injected into the carotid arteries mercury, and saw distinctly, that this metal passed not only into the smaller ramifications of the blood-vessels of the cerebral membranes, but also into an order of vessels, extremely minute, pellucid, and perfectly white. These vessels were by him subsequently denominated *nervo-lymphatics*, and from their great number he was of opinion, that they formed the greater part, if not the whole of the structure of the dura and pia maters. With regard to their source, a number of assistants and disinterested spectators were satisfied without having recourse to the use of a magnifier, that these vessels originated from the capillary arteries and terminated in the capillary veins. That the mineral passed directly into the substance of the encephalon, was satisfactorily demonstrated by the fact, that when the pia mater was elevated, it was clearly ascertained to be closely connected with the brain, by an infinite number of exceedingly minute vessels, which were filled with the mercury. Their arrangement was the same with regard to the arteries and veins as those of the cerebral membranes. He injected the lungs of a calf that had been recently killed with the same metal, which passed with great facility from the arteries into the veins, and from the former into the *nervo-lymphatics* of Vieussens. A close and beautiful mesh-work was thus formed, which appeared to fill up completely the unoccupied spaces between the arterial and venous capillaries. The injection was thrown into the pulmonary artery, and as the extremities of its capillary ramifications are directly united to those of the pulmonary veins, it is manifest, that the mercury could not have passed through those of the former into the pulmonary vesicles; therefore, we are necessarily constrained to conclude, that it must have passed through the nervo-lymphatics, which originate from the pulmonary capillary arteries and terminate in the pulmonary vesicles. These vessels were denominated by Vieussens *arterial nervo-lymphatics*, because they originated from the ca-

illary arteries, and also, because he discovered a number of nervous filaments, that came from the eighth pair and intercostals, to terminate in their parietes.

The vena portarum was injected with mercury by Vieussens. This metal passed into the membrane of the liver, and the small white vessels, of which it appeared to be composed, were arranged in small squares that communicated with one another. The spaces between these squares were filled with an extremely delicate membrane which was exclusively made up of *nervo-lymphatics*, of an almost inconceivable minuteness. When this membrane was separated from the liver, a very considerable number of short, but strong filaments, that were filled with mercury, and which closely connected the former with the latter, were quite demonstrable. The metal passed into the pori biliarii, and also into a number of filaments that passed from the hepatic veins into the capsule of Glisson. These vessels were also discovered in the uterus, mesentery, reservoir of Pecquet and even in the placenta. With regard to the organ last mentioned, it may be remarked that Littre in 1701 published an essay, in which he attempted, and I think successfully, to prove the existence of glands in the placenta. In the year 1716, it was asserted by Rouhauht, and since that time by Albinus, the Leyden professor of anatomy and surgery, that they believed the placenta to be formed altogether of vessels and cellular tissue. It is needless to say, that if this organ abounds, as these authors affirm, with cellular tissue, that it abounds also to an equal extent with absorbent vessels.

To place the truth of his discoveries upon a more substantial foundation, Vieussens varied his mode of investigation. From the body of a girl eight years of age, recently deceased, he extracted the stomach, placed ligatures upon the pyloric orifice and principal vessels—washed it clean—filled it with the alcoholic tincture of saffron, and then closed the cardiac orifice. In two hours, its parietes and vessels assumed a yellow colour. At the expiration of twenty-four hours the stomach was emptied—inflated with air and then permitted to dry. In this state it was exposed to a strong light, when it exhibited different

shades of yellow. Thus, the capillary blood-vessels were of a reddish yellow; the absorbents of a beautiful deep yellow; and there was another class of vessels that appeared to be still more minute, which were filled with a clear yellow substance, with slight and almost inappreciable variations, that seemed to arise from an admixture of the yellow matter with lymph. It was a fact, clearly and decisively established, that those vessels which were of the lightest yellow hue, and which appeared to contain the smallest quantity of the alcoholic tincture, originated from those of a reddish yellow.

The existence of the arterial absorbo-exhalents does not rest upon the uncorroborated evidence furnished by the anatomical researches of the incomparable Vieussens:—Hovius, a man distinguished for his attainments in science, and for his abilities as an anatomist, undertook a short time after the period rendered illustrious in the annals of microscopic anatomy by the discoveries of Vieussens, to inject the vessels of the eye. When the mercury had reached the point at which he supposed the arterial capillaries should become veins, he discovered that they divided into an immense number of smaller vessels—these again, were subdivided into more minute capillaries, and finally became veins. At the point at which these extremely minute subdivisions took place, there originated an astonishing number of clear pellucid vessels, which after pervading the different parts, terminated in the veins. Similar observations were made by Helvetius, Hecquet and Hamberger, all men celebrated during the eighteenth century for their attainments in the science of anatomy. Towards the close even of the last century, the researches of Bleuland operate in complete confirmation of the discoveries of Vieussens. He not only succeeded in injecting the absorbents which originate from the capillary arteries, but he made their existence and source matters of ocular demonstration. While he was yet a pupil of Sandifort and Van Doeveren, he frequently saw the injections of his distinguished preceptors pass from the arteries, not only into the veins, but into the excretory ducts and even into the absorbents. To satisfy himself of the channels, through the instrumentality

of which these communications were established, he prepared two different species of injection. One was red, and so remarkably subtile and insinuating, as to pass with facility into the most minute capillary blood-vessels, the colouring matter of which was insoluble. The other was white, but not so subtile or insinuating, the colouring matter of which could be readily reduced to a state of the utmost solution. When these two fluids were mixed together, and thrown into an artery, he expected that the latter would be able to penetrate into vessels that would be too minute to admit the former. Before these fluids were injected, he ascertained the precise amount of power it would require, to make fluids under ordinary circumstances, mechanically transude through the inorganic pores of the tissues. This precaution was observed, that, from this circumstance, no objection should be raised against the fairness and conclusiveness of his researches. Having settled these preliminaries, he selected a portion of intestine, the vessels of which he injected. Into the veins he threw a blue fluid, and into the arteries the mixture, the nature of which we have just pointed out. As he anticipated, (using the microscope,) he observed a great multitude of very minute vessels that received the white fluid only, and which manifestly originated from the arterial capillaries. Their number was so great, that it was impossible to put the point of a pin down without puncturing them. A coloured fluid was by Mascagni injected into an artery which passed entire and unaltered into the corresponding veins; while the injection, without a particle of the colouring principle, insinuated itself into the exhalents. He intercepted in a living animal, between two ligatures, the blood contained in an artery, which was deprived, he says, in a few hours of its most serous part. Doubtless, this fluid could not have been removed, but through the direct agency of the class of vessels, the existence of which we are now endeavouring to establish. "I threw," says Dr. Godman, "into the left carotid artery, melted tallow, highly coloured with king's yellow. The syringe was filled and emptied through the tube into the artery three or four times. On examination, I was much pleased to find, that the fluid had returned through

the veins, so as to fill them very perfectly; but on more attentive observation, my surprise was increased, by discovering that the smallest veins in both arms distinguishable by the naked eye, were filled with a material differing in colour from that contained in the arteries. In fact, the colouring matter was separated entirely from the tallow during its passage from the arteries to the veins: in the arteries, the colour was a rich yellow, deepening as it aproached their extremities, and in the the veins, the pure white tallow was entirely free from admixture." This talented anatomist, has either made unintentionally an erroneous statement, or has mistaken the character of the vessels which he imagined he filled with "*pure white tallow.*" He tells us, he "*was much pleased to find that the fluid had returned through the veins,*" but that his "*surprise was increased by discovering that the smallest veins*"—"*were filled with a material differing in colour from that contained in the arteries.*" From this we must conclude, that when he ascertained that he had made a successful injection of the veins from the arteries, that he observed nothing peculiar in the substance with which the former were filled, for he only expresses his surprise when he discovered, that what he calls the "*smallest veins*"—"*distinguishable by the naked eye,*" were filled with a colourless fluid. Indeed, he never mentions the tallow being in its pure state except in capillary vessels, while he speaks of the veins being filled "*very perfectly*" without this circumstance ever attracting his attention. The inference is therefore fair and legitimate, that the first veins that met his observation were injected with tallow coloured with "*king's yellow.*" This being the case, we are at a loss to account for the "*king's yellow*" being in the larger veins without having passed through those of small capillary calibre. How then can this much esteemed anatomist say, that "*the colouring matter was separated entirely from the tallow during its passage from the arteries to the veins?*" That the injection passed unaltered from the former into the latter, I think is very manifest, and also that the separation between "*the colouring matter*" and "*the tallow,*" took place in "*its passage from the arteries to the*" arterial absorbo-exhalents.

In addition to the evidence already furnished in the preceding pages, we deem it important to remark some other communications between the arteries, and the absorbents and excretory ducts, which have been pointed out by authors, before we draw to a close the evidence in favor of the arterial absorbo-exhalents. Thus, it is asserted by Malpighi and Ruysch, that the glands are a compound of capillary vessels—white and pellucid in character, which originate from the arteries and which they have rendered visible to the naked eye by injection. This is substantially the opinion of Berger, Albinus, Beddevole, Chirac and some of the most celebrated of the more modern anatomists. By Heister, the glands are considered follicular, in which terminate a multitude of minute white vessels, that come from the capillary arteries. By Mauchart, this opinion with a slight modification is adopted. By Ferrein it is said, "that the vessels which are denominated glandular are a marvellous assemblage of white cylindrical tubes, differently folded upon each other, and which may be demonstrated in the kidneys, liver, &c." Indeed, this author, as well as Gunzius, admits the existence of two classes of absorbents which they denominate, arterial lymphatic vessels, and venous lymphatic vessels; the latter of whom, has given plates by which they are represented.

By injecting an artery, Meckel says we may fill the absorbents in its vicinity. By Barclay we are told, that immediately after death, he has often, and with great comparative ease in the dog, injected the absorbents of the cord from the spermatic artery, and without any evidence of rupture as far as he could discover. By Portal we are assured, that an injection thrown into the renal arteries, readily passes into the ureters, and that frequently it has, when thrown into the hepatic artery passed into the hepatic duct. From Cruikshank we learn, that from the artery he has injected the ureter and pelvis of the kidney without rupture. In three different kidneys he injected from the ureters, the tubuli uriniferi for a considerable distance along the mamillæ, and in one case, a number of veins on the external surface of the kidney, were evidently filled with the injection. Albinus, as we are informed by Haller, in injecting

the pancreatic and salivary arteries, filled also the excretory ducts of these organs.

In conclusion, we will remark, that in Horner's Anatomy a peculiarity is mentioned with regard to the tunica arachnoidea of the brain, which, while it demonstrates the necessity of a class of vessels like those under consideration, proves that neither the blood-vessels or nerves are the direct agents of secretion. This membrane he tells us, is exceedingly delicate; is always transparent; and is furnished with neither red blood-vessels or nerves. It secretes a sort of halitus or synovia, which facilitates the motion of the brain, and prevents it from adhering. Occasionally, this secretion is so much augmented as to constitute a genuine dropsy.

Venous absorbo-exhalents. Although the anatomical researches of Ruysch, would reflect considerable light upon the subject we now intend investigating, we deem it prudent to omit their particular consideration, in order to place before the reader, the result of the labours of more modern and better known, but not more deservedly celebrated anatomists. This would be impossible, for his works stand as an imperishable monument of his industry and talents, and though the brightness of his fame may have been obscured by the dark lowering of a passing cloud, the most recent developements in the science of anatomy give sure indication, that his labours will yet be as familiar to the student, as he was with the structure of the human body.

When M. Ribes injected the crural vein, the fluid penetrated not only into the cellular tissue, which we have shown is made up almost exclusively of absorbent vessels, but into the skin and to the whole dermoid organ imparted the colour of the fluid injected. The essence of turpentine coloured black, was injected into the ventral portal vein, which passed into its ramifications; into the peritoneum; into the cellular tissue situated between the peritoneum and intestinal tube; swelled the bowels and the villi of the internal tunic to such a degree, that when floating in water, they had the appearance of black moss. Kaaw Boerhaave opened the chest and abdomen of a dog immediately after his dissolution; he emptied the stomach, and

filled it with warm water. This fluid soon penetrated into the great gastric veins; passed through the vena portarum; traversed the liver; entered the vena cava; and distended the right auricle and ventricle of the heart. We are informed by Blizard, that on two different occasions, he distinctly saw absorbents terminate directly in the iliac veins. It is remarked by Cruikshank, that there is not an anatomist possessed of address in the art of injecting the arteries and veins, who has not succeeded, through these vessels, in filling also the absorbents. He on several occasions succeeded in injecting the thoracic duct and a considerable number of absorbents through the umbilical vein in children. It is asserted by Trew, that when mercury was poured into the thoracic duct, it passed into the veins of the brain and into the arteries of the pia-mater. Haller, not satisfied with giving his assent to the statement of Trew, and also of a number of other anatomists who contended for the termination of absorbents in the veins, assures us, that he was an eye witness of them himself. On this subject a most important fact is furnished by M. Ribes, who says, that for a long time he conjectured the existence of absorbent vessels in the medullary cavity of the bones, which absorbed the marrow, and terminated in the veins. Experiment, he says, has partly verified his conjecture in disclosing to him the fact, that the superficial absorbents of the liver may be filled by injecting the hepatic veins. This experiment, several times repeated, and uniformly with the same success, proves incontestibly, that all the absorbents do not terminate in the thoracic duct. Vieussens has seen absorbents terminate in the veins; this is an anatomical fact admitted by Senac, for he carefully distinguishes those absorbents that terminate directly in the veins, from those that terminate in the thoracic duct. Numerous communications have been pointed out between the venous and absorbent systems by Highmore, Wepfer, Stenon, Nuck, Ruysch and many other distinguished anatomists.

An interesting experiment was performed by Caldini, which strikingly illustrates the point under consideration. After feeding a lamb to satiety, he placed a ligature upon the vein which corresponds to our subclavian, and another which was

drawn very tight upon the mesentery at its origin near the lumbar vertebræ. The lacteals and lymphatics between the ligatures, as well as the absorbents of the posterior extremities, became from distension, distinctly visible. The former were soon distended, but as soon began to subside, and eventually, entirely disappeared, evincing most decisively, that the chyle and lymph had found out some other channel into the circulation.

With regard to the existence of a communication between the lacteals and the vena portarum, it was not only suspected by the older anatomists, but was by them actually demonstrated. Thus it was observed by Walæus, that when the lacteals were tied, and the chyle prevented from going into the thoracic duct, it went directly into the vena portarum. It was discovered by Rosen Wallerius, Meckel, Lobstein and Lindner, that when the absorbents of the stomach, bowels and spleen are filled with quick-silver, that it passes into the vena portarum. By Sir Astley Cooper mercury was detected in the same vessel, after it had been injected into the mesenteric absorbents. The same fact is testified to by Fohmann, when the intestinal absorbents are filled. By Tiedmann and Gmelin, to ascertain if this communication does actually exist, the absorbents of the intestinal tube of two dogs, one horse, one cow, and three human bodies were filled with quicksilver. The injection was begun soon after death, and carried on with the greatest care, and it was discovered that without the application of any external force, the metal was enabled to reach the branches of the mesenteric veins, and the vena portarum. A chyle-like fluid has been frequently detected in the blood of the vena portarum, which phenomenon cannot be readily explained, unless we admit the existence of a vascular communication, between the lacteals and that vein. This circumstance has been frequently observed by a great number of anatomists, among whom may be mentioned the distinguished names of Harvey, Swammerdam, Menghini, Meckel, Brendel, Cruikshank and Alexander Monro.

Let us now ascertain, the result of the anatomical researches that have been instituted on this subject, not only since the com-

mencement of the present century, but even during the short lapse of the last ten years. The most modern anatomists have not been idle;—by them, that beautiful, interesting and important branch of their science denominated minute anatomy has not been abandoned as a barren, dreary, unproductive waste. The names of Meckel, Fohmann, Lauth and Lippi, will always rank high among the cultivators of this department of science.

The experiments of Dr. Meckel, consisted in the injection of mercury into various absorbent vessels and minute secreting cavities; and he found, that a direct communication takes place between such cavities and absorbents, and the veins in immediate connection with them. He traced this communication particularly in the breast, in the liver and in the bladder: and he thus accounts for the ready passage which bile finds into the blood, when the ductus choledochus is obstructed as in jaundice; and the urinous fluid which is often thrown from the axillæ and other organs upon a suppression of the natural secretion. M. Fohmann, a distinguished anatomist of Heidelberg in 1820, declared the existence of a direct communication between the absorbents and the veins, not only in their primary and capillary plexus, but also in the interior of the lymphatic ganglions. He was soon succeeded in this field of inquiry by M. Lauth of Strasburg, who went to Germany to learn of M. Fohmann his mode of injection, and who announced the same facts in his dissertation at the school of Strasburg, 1824. By this communication of the absorbents and veins, M. Lauth explains how an injection thrown into an artery passes into the absorbents, without effusion into the cellullar tissue: the injection, he supposes, but most erroneously, passes from the arteries into the veins, and from thence by a retrograde motion along what we have denominated venous absorbo-exhalents into the lymphatics. Mr. Beclard admits the correctness and accuracy of the researches of Messrs. Fohmann and Lauth, at least with regard to the communications which take place in the lymphatic ganglions. This author supports his opinion by what he has observed in birds. When the lymphatic ganglions are wanting, their place is supplied by plexus or vascular net-works, in which he has distinctly seen the

lymphatic vessels open into the veins. In 1825, M. Lippi of Florence published a work upon the anatomy of the chyliferous and lymphatic vessels, in which this communication is fully and satisfactorily established. He does not consider the communication of the absorbents with the capillary veins and in the ganglions a matter of doubt: this he considers demonstrated, and directs his attention more particularly to those that terminate in the large veins. According to M. Lippi, there exist numerous communications between the lymphatic vessels of the abdomen and the inferior vena cava and all its branches;—the lumbar and spermatic veins, the vena azygos &c. These communications, he says, are so numerous, that every vein receives a lymphatic vessel. About the second and third lumbar vertebræ these lymphatic vessels manifestly divide into two orders; one ascending which terminates in the thoracic canal, and the other descending which opens into the renal veins and into the pelvis of the kidney.

It has been proven by the experiments of Duverney and Donald Monro that there must necessarily exist some other channel besides the thoracic duct into the circulation. Thus, Mr. Duverney by a ligature upon the subclavian effectually prevented the chyle from going into the circulation through the thoracic duct. The animal lived fifteen days. A ligature was placed upon the thoracic duct of certain animals by Dr. Monro, in order to exclude if possible the chyle from entering the circulation. In this, however, he was disappointed, for after having fed the animals upon which he experimented some time with madder, they were killed and examined, when it was ascertained, that the serum of the blood and the bones were strongly impregnated with this substance.

Independently of the researches of the anatomist, and the experiments of the physiologist, the powers of reason, associated with the cooperating energies of abnormal phenomena, are amply sufficient to force the most decisive conviction, upon the mind of the unprejudiced, that the venous absorbo-exhalents have a real bona fide existence in the animal economy. Thus, the great and striking difference observable between the capacity

of the absorbents aggregately considered, and that of those that terminate in the superior vena cava, by Wepfer was long since noticed, illustrated, and the necessity of some other channel into the circulation forcibly pointed out. This consideration is abundantly sufficient to convince us, that the recognized termination of the absorbent system, in the circulation, is not ample enough to answer the purposes of the economy, or to explain the numerous phenomena that arise in the various normal and abnormal states of the system. We are already aware of the manner in which the absorbents abound throughout every structure of the body; we know that they enter as an indispensable element in the organization of every tissue; we know that they are spread throughout the extensive surfaces of the dermoid organ, of the pulmonary mucous membrane, and of the lining of the whole tract of the alimemtary tube; we know that from every serous cavity, there are fluids to be removed, and that in the process of decomposition, the solid structure of the body is to be broken down. Without taking into consideration the chyle which it is supposed flows through the same tube, can we be so extravagant in our speculations as to imagine, that these numerous and abundant fluids, from those diversified sources, can by any species of arrangement, either of dilatation on the part of the vessels, or condensation on that of their supposed contents, be forced through the lymphatics that terminate in the large veins about the right side of the heart, the largest of which ordinarily does not exceed in size a common crow quill?

Moreover, if these were the only outlets of the absorbent system, the amount of venous blood would be greater, in proportion to the size of the vessel, between the point at which the thoracic duct enters the subclavian vein and the right auricle of the heart, than in the remainder of the venous circulation, while it would be greater only in this place, than the arterial blood, and less in the remainder of the venous system, in consequence of the losses sustained by the arterial blood, in furnishing the materials out of which the secretions are elaborated. On this subject Mr. Ribes remarks, that in a ratio corresponding with the size of the vessel, the quantity of blood in one vein is not greater than

that in another, but that the aggregate amount of venous is superior to that of the arterial blood. The superior capacity of the veins is admitted by Borelli, Sauvages, Haller and Keil, and as they are necessarily a plenum, the quantity of blood they circulate must be greater than that by the arteries. As the arterial blood, however, is perpetually sustaining losses, the amount of venous blood should be less than that of the arterial instead of being greater, if the venous circulation is not augmented from any other source than the admitted terminations of the absorbent system.

The great variety of unassimilated substances to be found frequently in the veins generally, but in the vena portarum in particular, furnishes conclusive proof, (whether or not we admit the existence of venous absorbo-exhalents) that there are avenues to the venous circulation, independently of those that terminate in the superior vena cava. Under the controul of abnormal action, these substances enter the veins, and render the blood of one part, different in some respect, from that of another. The lymph and chyle never present such numerous varieties, nor such discrepant qualities as are sometimes exhibited by the venous blood. It is a distinguished attribute of the action of the lymphatic absorbo-exhalents radically to change the properties of the substances upon which they exert their energies, while in the veins, unassimilated articles are found unaltered in any respect whatever. This is the result of the multiplied observations of Mr. Ribes, which makes the presumption strong in favour of the idea, that in conducting them into the venous circulation the lymphatic absorbo-exhalents exercise no agency.

Thus, pure pus has frequently been detected in the veins, when it was absolutely impossible to discover any circumstance, that tended to induce the belief, that it had proceeded from inflammation of these vessels. Most generally, the existence of an abscess in the vicinity, has satisfactorily indicated its true source. By professor Chaussier we are informed, that he has frequently observed a sanious matter in the veins of the abdominal viscera; by Mr. Velpeau, several cases are recorded, in which

pus was found in the blood-vessels and effused in different parts of the body, and this too, when he was unable to observe any symptoms of inflammation during life, or to detect upon dissection after death any traces of the existence of that kind of action: and by Mr. Ribes we are told, that he has frequently seen pus mingled with the venous blood, when it had to all appearance circulated for some time with that fluid. He has also detected shining particles floating in it that bore the aspect of globules of fat. Nothing of this kind he assures us, has he ever observed in the fluids circulated by the arteries or the lymphatic absorbo-exhalents.

The existence of these unassimilated articles in the venous blood is admitted by some very celebrated authors: they contend however, that in conducting them into the veins the absorbents exercise no agency. These are the partizans of the doctrine which ascribes to the veins the faculty of absorption. Let us inquire into the nature and validity of the evidence upon which this hypothesis is predicated.

Venous Absorption. The hypothetical opinions espoused and defended by the modern disciples of the celebrated Boerhaave on the subject of absorption, do not present that perfect accordance, which is always a plausible argument in favour of the truth of a hypothesis. Thus by Tommasini, a celebrated Italian, the absorbing power of the veins is limited in its action to the blood exclusively which has reached the minute extremities of the capillary arteries. This suggestion, although perfectly unauthorized, induced Martini to suppose that in the experiments of some distinguished physiologists, there was a solution of continuity, by which the open extremities of the veins were unable to absorb. By M. Magendie, the faculty of absorption is positively denied the lymphatics, while it is inconsiderately, and with unphilosophic precipitancy attributed to the veins.

The opinion of Tommasini is demonstrated to be false and untenable by the facts that the arteries and veins are continuous, and that the blood in the veins is under the control of the action of the left side of the heart. The supposition of Professor

Martini, is equally idle and preposterous. Thus it is remarked by Schreger, that the veins are so little qualified for the function of absorption, that when they have been wounded or completely divided, he has never been able to induce them to take up any substance when applied to their open orifices, while the absorbents in the same situation precisely, became readily distended. This is the result also of the researches of Oudeman upon the same subject. The opinion of M. Magendie has been more strenuously defended; has been presented to the public under a much more beguiling form; has succeeded in making a much greater number of proselytes; and has exercised a most deleterious practical influence upon the science of medicine: we, therefore, deem it important to take it into detailed and elaborate consideration.

This hypothesis, is predicated upon experiments performed by M. Magendie himself, and which he has also attempted to support by reasoning. He divided the thigh of a living animal, in such a manner, as to suffer it to remain attached to the body only by the crural artery and vein. The *upas tieute* was then inserted into the foot of the animal, and in four minutes its poisonous influence was manifested. The leg of another dog was entirely amputated at the thigh, and the barrels of two quills were inserted into the divided extremities of the crural artery and vein, in such a manner as to transmit the blood into the amputated extremity and to return it again back to the heart. In this experiment the effects of the poison were equally manifest. Upon the truth of these experiments rests the doctrine of venous absorption: we shall, therefore, endeavour to ascertain if they are entitled to the influence which they have exercised.

In the first place let us remark, that they have been repeated by other individuals fully competent to the task, who were unable to procure the success which he asserts attended his exertions. Thus, the hind leg of a dog was completely divided with the exception of the crural vein by Dr. Sommerville; an infusion of nux vomica was inserted into the amputated part, and although the circulation continued to be vigorous in the crural

vein during the space of five minutes, not the least appreciable deleterious impression was made upon the animal. A leaf of tobacco moistened in water, was then applied to the incision as ineffectually. To prove the genuineness of the poison, he placed it upon the stump of the amputated limb, from which resulted convulsions in five, and death in fifty minutes. With great care, skill, and judgment, the experiments of M. Magendie were repeated by Dr. Hubbard, but without his being able to confirm them. In my presence, they were repeated by a gentleman of great address and intelligence, who was moreover particularly partial to the opinions of the French physiologist, and although he viewed the results obtained with much complacency, no individual present was satisfied but himself. Feeling a deep interest in the settlement of this subject, I determined to repeat them myself and with every precaution to ensure success, but met with an entire failure. If in future, it should appear necessary, these experiments shall be in a circumstantial manner given to the public.

Though many individuals have failed in the attempts which have been made to procure the results obtained by M. Magendie, we do not consider this sufficient reason to authorize a doubt, much less a disbelief in the correctness of his statement. Few men possess the address and genius for experimenting, so characteristic of that distinguished physiologist: it is therefore, easy to conceive that results may have been obtained by him, not to be procured in less experienced hands. It is not necessary to invalidate the correctness of his experiments, to prove that they do not establish the truth of the doctrine of venous absorption.

In order to explain the experiment in which the crural artery and vein were divided, and this is the one in which the most reliance is placed, it is altogether unnecessary to ascribe the faculty of absorption to the veins. When the poison was inserted into the foot of the animal, it was manifestly mingled in a direct manner with the blood. To accomplish his destruction therefore, all that would be necessary would be the continuance of the circulation in the amputated limb. This I believe has

never been denied. To have answered the purpose for which this experiment was contrived, it is absolutely necessary, that the part should have sustained no lesion of any kind whatever. The *upas tieute* should have been simply rubbed upon the part, and the foot should have been plunged into a decoction of nux vomica. The experiments of Fontana go to prove that the poisons employed by M. Magendie acted directly upon the blood. This distinguished individual has investigated the subject of poisons more extensively than any other physiologist: he was induced to conclude from the results of his numerous and diversified researches, that poison does not get into the system, but exerts an immediate influence upon the blood itself. This may possibly be true, and if the blood should be poisoned, it is very certain, that when it goes into the system the animal must inevitably perish. It is therefore, necessary to ascertain the modus operandi of poisons, before any deduction can be made from the experiments of M. Magendie.

To suppose, for the sake of argument, that the poison was neither mingled with the blood, nor exercised according to the opinion of Fontana, an immediate influence upon that fluid, would it be legitimate to infer that the veins are endowed with the faculty of absorption? Unqestionably not; for the characteristic attributes of the venous and absorbent tissues, are in direct contradiction to an idea so grossly gratuitous and unfounded. The amputated limb, totally cut off from all connection with the brain and nervous system, the circulation of the blood through it purely mechanical, the arteries and veins in their normal state nearly inert, and now necessarily dead, is it reasonable to suppose the veins thus situated are capable of performing any function dependent upon their vital attributes? To explain the absorption of the poison, if it really was absorbed, it is necessary to have recourse to some tissue more retentive of life than that of the veins. Are we in possession of any facts to sustain the belief, that they are more tenacious of this principle than the tissues generally? We have no reason to suppose that they ever under any circumstances, survive the extinction of life in other parts. This is not the case, however,

with the vessels of the absorbent system, they are highly irritable; eminently endowed with many other active vital properties; and among their most distinguished attributes is their peculiar retentiveness of life, as is illustrated by their acting frequently for hours and even days after the final cessation of the functions of respiration and of the circulation of the blood.

If we, therefore, attribute the absorption of the poison to the agency of the venous absorbo-exhalents, we shall be able, perhaps, to assign a satisfactory reason for the failures with which some physiologists have met in attempting to repeat the experiments of M. Magendie, while others have succeeded. Thus this retentiveness of life is not uniformly manifested by the absorbents: this principle is frequently extinguished in them synchronously with the dissolution of the other tissues. In the failures above alluded to, this possibly may have happened.

With an additional remark, we shall conclude our strictures upon the experiment of M. Magendie. For this experiment to have been an *experimentum crucis*, as it has been preposterously denominated, the poison should have been inserted into a part, that had no other connection with the body than through the medium of absorbent vessels, and then, if no impression had been made upon the system, its author might with propriety have attributed the faculty of absorption to the veins. But as it is now exhibited to the public, with all its imperfections, any inference deduced from it in favour of venous absorption, stands on apochryphal authority.

The reasoning of M. Magendie is as sophistical as his experiments are radically defective. His arguments are invariably of a negative character;—he has not urged one that amounts to any thing bearing even a remote resemblance to demonstration. Thus, it is remarked, that half the animal kingdom, and all those of the invertebrated class, are totally destitute of absorbents, and that absorption in them, is accomplished through the direct agency of the veins. Because they have not been demonstrated is no proof that they do not exist. The fact moreover, that in them, the process of absorption is carried on, is sufficient, doubtless, to prove that they have also a particular order of vessels intended

for that purpose. This is rendered the more certain, as there is no analogy of a positive character, in favour of venous absorption, while evidence of this sort can be adduced in the greatest abundance, in support of the absorption of the lymphatic vessels. Besides, it is perfectly impossible to conceive of any species of animal organization, into which absorbent vessels do not enter as an essential element. But to admit the argument in all its force, it can have no other tendency than to render it probable that the veins absorb, while it can exercise no influence in invalidating the doctrine of lymphatic absorption. Indeed, if the reasoning of M. Magendie be authorized logic, we might with propriety turn his arguments against himself. For how many parts are there, in which absorption is unequivocally carried on, and which are nevertheless, entirely destitute of veins?

By M. M. Ribes and Meckel it is asserted, that the absorbent vessels do not communicate as freely with the surfaces as the veins. This statement is gratuitous, as the reader is undoubtedly convinced, by what was said when treating of the structure of the different tissues. If it were true, however, what good purpose could such an arrangement of the veins answer? Their professed design is to return the blood to the heart; why, therefore, should they open upon the skin, the mucous surface of the stomach or bowels? Do the arteries ever pour blood upon the skin or into the alimentary tube, in order to be absorbed by the veins, and then returned to the heart? A free and open communication with the surfaces, however, does not promote absorption by the veins. We have already seen, that when the absorbents and veins, were placed precisely in the same situation, with free and open mouths the former readily absorbed, while the latter altogether declined that function.

It is asserted, that the mesenteric veins participate as largely in the structure of the villi of the mucous membrane of the bowels, as the lacteals, and have also as free and open orifices into the intestines. An injection thrown into the vena portarum by Lieberkuhn, passed into the bowels through these villi, and M. Ribes obtained the same result by throwing into it mercury or the essence of turpentine coloured black. The existence of

veins in the structure of the intestinal villi is a conjecture and nothing more: they have never been demonstrated: indeed, the existence in them even of lacteals is denied by some anatomists. The fact, however, that the best informed anatomists, admit the existence of absorbents in the intestinal villi, while no one has been able to find veins in them, proves clearly that the communication above indicated, took place through the venous absorbo-exhalents.

Chyle it is affirmed, has been frequently observed by M. M. Gmelin and Tiedemann in the mesenteric veins, and in an experiment performed by Swammerdam, which consisted in tying the mesenteric veins in a living animal during digestion, he imagined he saw whitish, chylous striæ in them It is not certain that this was chyle, nor is it asserted; it is only denominated whitish striæ. It is perfectly impossible for this to have been real chyle if it was absorbed by veins; for this fluid does not exist in the duodenum—it is only to be found in the lacteals, and the mesenteric veins have neither the structure nor action, by which the chyliferous vessels are enabled to elaborate the chyle out of the chyme. It is possible, that in these veins may have terminated a chyliferous vessel, as this is what often takes place. Thus, it is asserted by Valœus, that having tied the intestinal absorbents, the chyle passed directly into the vena portarum. This is confirmed by Rosen, J. F. Meckel and Lobstein; and M. Lippi, as we have already seen, asserts that the chyliferous vessels terminate in the veins, not only while they traverse the mesentery, and before they plunge into the ganglions, but also in the mesenteric ganglions.

It is said that venous absorption is proved by the fact, that the colouring, odorous and saline substances, that have been exposed to the action of intestinal absorption, are more frequently found in the vena portarum than in the chyliferous vessels. The reason of this is very obvious: for while it is very difficult to modify the elective sensibility of the latter, so as to enable them to admit unassimilated substances, that of the venous absorbo-exhalents, not being so exquisite, is much more readily brought into relation with foreign articles. The design of this differ-

and he remarks, that the majority survived more than fifteen days, and that during the two first, there were no deaths, although when they were opened, he found the thoracic duct burst, and the chyle effused into the abdomen. If these experiments had been adduced to prove, that the chyliferous vessels communicate with the circulation, through other channels than that of the thoracic duct, the selection would be considered judicious, and would perhaps have accomplished the purpose for which they were designed. When, however, they are brought forward to prove, that chylification is a function of the veins, and that absorption is not a faculty of the lacteals, the selection is injudicious, and entirely insufficient for the objects to be attained by them. The experiments of M. Flandrin were carefully repeated by M. Dupuytren; and when it was impossible for the chyle to enter the circulation, the animals invariably died in a short time. If, indeed, it be admitted, that the mesenteric veins do absorb, and that they extract from the chyme a product useful to the economy, it is perfectly impossible for this product to be chyle—to possess any of its essential attributes—or to nourish the body, as these vessels are not endowed with the particular structure and consequently, cannot exercise the peculiar action of the chyliferous vessels. This product in the mesenteric veins, must of necessity be venous blood. That the lacteals do absorb chyme, and out of it elaborate chyle; that they are endowed with a structure, and action particulaly adapted to the production of these effects; and that in these points of view they differ radically from the mesenteric veins, are facts which the enlightened of the profession, who stand uncommitted, will not pretend to deny. It is, therefore, supremely nugatory and idle to contend, that vessels, so essentially unlike, were designed by nature to perform the same function.

[TO BE CONTINUED.]

ART. IV.—*Cases of Lithotomy in which the Stones were dependent on the presence of Bone in the Bladder.* BY GEORGE W. CAMPBELL M. D. of Columbia, Tennessee.

THE operation of Lithotomy is becoming so common, that it is seldom heard of beyond the common limits of one's practice. The disease of stone in the bladder has presented itself frequently in my practice: two cases, however, which have occurred very recently, present, to me at least, something new in the history of this disease; on which account, I place them before the public.

Case 1. George Wallis, of Wayne County, aged twenty-six years—of scrofulous habit—when fifteen years old, was attacked with pain and swelling of the thigh and groin of the right side, which lasted about two months and gradually wore off, leaving, however, slight soreness with a little halt in his walk. At the end of two years the swelling and inflammation returned and resulted in an abscess in the right groin, in six weeks.

The abscess formed on a line from the anterior superior spinous process of the os ilium, to the symphysis pubis, one third of the distance from the latter.

The abscess remained open about two years, the patient still attending to his ordinary occupations. Spiculæ of bone of various dimensions were discharged. The abscess closed up in the fall of the year, leaving the patient, as he supposed, convalescent. In the spring following, while in usual health, on attempting to urinate in the morning, without any premonitory symptoms a difficulty in passing urine was experienced, followed by a profuse discharge of blood, which passed in clots, so as occasionally to interrupt the flow of the blood and urine, attended with comparatively little pain. Those bloody discharges gradually gave place to discharges of mucus and pus, such as were discharged from the abscess that had existed in the groin. Sev-

eral pieces of bone, from half an inch to two inches long, were discharged from the bladder, through the urethra while urinating. Some pieces resembled those discharged from the groin; others were covered with earthy matter, owing probably to their having been detained a longer time in the bladder. No bone or calculus has been discharged for the last five or six years. Very considerable discharges of mucus and pus, and occasionally blood, have been kept up.

About three years past, an abscess formed just below the short ribs on the right side, which discharged matter very freely all summer, and closed up. The glands about the neck and clavicle, of the same side, were inflamed and swollen, but did not suppurate.

Soon after the first discharge of blood, symptoms of stone in the bladder made their appearance, and soon became very violent, attended with great pain in the loins; occasionally with an entire loss of the use of the lower extremities; the urine either continually dripping, or entirely obstructed; distressing gripings of the bowels, with tension and soreness of the abdomen, extending to the groin and thigh of the right side. Excruciating pain was felt in the region of the bladder and deep in the groin, on any sudden motion of the body on the pelvis.

While in this deplorable condition the patient was advised to go to Alabama in search of a doctor, who, in the patient's neighbourhood, had some reputed fame for curing the gravel. The patient after a painful journey of some days, was with the doctor, who pronounced his disease to be some affection of the kidneys, probably diabetes, and sent him home with his pockets stuft with *uva ursi*, assuring him he would be well in a short time.

The patient continued to grow worse, until he found his way to this place, and put himself under my care. After hearing the history which he gave of his case, I mentioned to those present, that we should find bone and stone both in the bladder. On introducing the sound, the bladder appeared to be literally filled with stony substances. I felt satisfied, from the examination with the sound, that there were several stones in the bladder, or a stone formed in irregular clusters.

The 24th of December 1829 was appointed for the operation. Anticipating difficulties, I made the incision large, used the largest gorget but one in the case of instruments; however, the bladder being nearly filled with the stone, the gorget was not plunged into it as in common cases. The stone was immediately laid hold of with the forceps, but it was immoveable. I enlarged the incision in the bladder, with a scalpel: still the stone remained firm. I introduced the large forceps for breaking the stone, and succeeded very soon in breaking off one of the protuberances, which was as large as stones commonly met with in the bladder. After extracting three such pieces, one still larger remained, somewhat under the arch of the pubis and to the right side. After a few unsuccessful efforts, I succeeded in bringing it down, with a scoop, so as to grasp it with the forceps. There was attached to this portion of the stone, a spicula of bone, an inch in length, rough and irregular in its form.

This part of the bone had never got into the bladder, but was lodged in the cellular substance in the neighbourhood of, and external to the bladder, while the other end of the bone had penetrated the bladder and was covered with, and converted into calculous matter. In extracting the last portion of stone, the bladder was very much lacerated. The position happened to be unfavourable: while the body of the stone constituted a lever, the bladder was a fulcrum, which caused the portion of bone external to the bladder to change its position tearing the soft parts about the bladder and the bladder itself, as the body of the stone was drawn down. The laceration, from extracting the bone, was productive of more hæmorrhage than the incisions with the scalpel and gorget. I could easily put my finger into the opening. The patient became faint several times during the operation, which consumed fully half an hour. After being laid in bed, some cordial drinks were administered, with warm applications to the extremities, frictions, &c. Took laudanum at night.

Dec. 25. Has fever—rested badly through the night—drinks cream of tartar and slippery-elm water—discharges blood and water from the wound—took oil at night. 26th. Oil operated —considerable soreness and swelling in the groin, extending

down the thigh—testicle very much swollen and painful—warm fomentations were applied—high fever.

27th. Less fever than yesterday. Inflammation and swelling subsiding—discharges bloody water—no healthy appearance in the wound—poultices applied to the perinæum, warm fomentations to the abdomen, &c.

January 1st. No appearance of suppuration in the wound—wound lined with a white earthy matter—allowed a more generous diet.

2d. Had chills through the night, attended with difficulty of breathing—took laudanum—has fever.

3d. Fever abated—complains of smarting in the wound from the flow of the urine—some appearance of suppuration—took oil.

4th. Oil operated well—discharge of pus and mucus from the wound—symptoms more favourable.

5th. Suppuration more copious—takes rice, milk, corn-bread, and a little meat—appetite good.

6th and 7th. Symptoms &c. the same.

8th. Had chills through the night—darting pains through the whole system—difficulty of breathing—suppuration stopt—symptoms all unfavourable—took a large tablespoonful of paregoric, 10 o'clock at night.

9th. Had a good night's rest—perspired freely—wound assumed a healthy appearance—urine passed occasionally through the urethra.

12th. Health improving—suppuration or sloughing copious—principally from the lacerated part of the bladder and neighbourhood of the bone and groin, originally diseased—appetite good—takes nourishing diet, with wine.

15th. Chills returned—suppuration stopt—fever—white stringy substances lodged in the wound—covered over with calculous matter—extracted in considerable quantities with the tweezers.

18th. Health greatly improved—eats hearty—takes no medicine—healthy suppuration—lumps of white membranous matter still passing—mucus holding in solution a considerable

amount of earthy substance. These plugs appear to be portions of the inner coat of the bladder, covered with mucous and gritty substance.

25th. Mucous discharge ceased—plugs still passing.

28th. All discharges from the bladder have ceased, except the urine which passes both ways. Probably the whole mucous coat of the bladder is now separated and discharged, and a new one formed in its place. The neck and even the whole inner surface of the bladder is more tender and sensitive at present, than before those membranous discharges took place, owing probably to this morbidly thickened coat, being separated from the more sensitive parts.

The patient was discharged on the 16th of February in good health and entirely well of the disease and operation, except a small opening in the perinæum, through which the urine escapes occasionally, passing, however, principally through the urethra, at the will of the patient. The tenderness of the bladder, and the probability of there still remaining some undetached portion of diseased substance, in or near the bladder, which, when detached, could not pass off through the urethra but be detained and afford a nucleus for the re-formation of stone, induced me to postpone any attempt to close the orifice. In the present state of the bladder, a catheter could not be worn a sufficient length of time, to heal up the fistulous opening in the perinæum. Several who were present at the operation took the portion of stone last extracted, to be the head of the os femoris, to which it bore considerable resemblance. The spicula of bone attached to it gave it the appearance of the neck of the femoris. The bones, however, I presume came from the pubic portion of the innominatum. While the disease was confined to the anterior superior portion of the bone, the exfoliated parts were discharged through the abscess in the groin; but as the disease extended to the inferior posterior portion, probably affecting the ramus of the os pubis, the detached bone found its way into the bladder, bringing with it blood and matter, furnishing nuclei for stone to form upon.

Case 2nd. In the summer of 1824, Jack, a negro boy, the property of a gentleman in this neighbourhood, fifteen years old, was attacked suddenly with pain in the heel—various remedies were applied, without effect, for ten days, when I was for the first time requested to visit him. I found him with a hot dry skin, furred tongue, tense pulse, constipated bowels, and complaining much of pain in the foot and knee, the latter a little swollen, with a loss of power to use the limb. On examination, I found the seat of disease, to be in the hip joint. I had him bled frequently, purged and blistered freely, until the local and general morbid symptoms disappeared. The patient went upon crutches for some time, and was able to attend to his ordinary employment in a few months. I heard no complaint from this boy until last June, (1829,) when he was sent to me by his master, to prescribe for his eyes, which were literally red with blood—attended with no pain, or obstruction to vision. This inflamed appearance of the eyes wore off in a few weeks, when he was again sent, on account of pain in the back, extending down the thighs—complained more of the right hip, than any other part—the right was the hip which was diseased in 1824. There were also discharges of blood from the urethra, with involuntary discharges of urine. About the first of January last, (1829,) in attempting to pass water, a scale of bone about the size of the little finger nail lodged at the orifice of the urethra, and was extracted by the boy himself, with the assistance of his knife and a common brass pin. So soon as I saw this piece of bone, I was more fully confirmed in the opinion, of his present symptoms being the effect of his old hip disease, which I had previously expressed to his master. Having but a short time since operated on Wallis, where the causes of stone, were similar to the symptoms here, I suspected stone in the bladder.

On the 27th of January, 1830, I operated on this boy and extracted a stone the size of a hen's egg. The stone was exceedingly rough, and so soft that it gave way in the grasp of the forceps. The stone presented different colours and textures—appearing in places, to be interspersed with animal mat-

ter. It had not been quite a year forming, if we date its existence from the first symptoms discovered; there being so much clotted blood and mucous retained in the bladder, together with an increase of calculous diathesis, that its growth was greatly facilitated. The inner coat of the bladder was considerably diseased—appeared thick and wrinkled. The mucous discharge was very considerable—I was not able to detect any bone in the bladder. The patient appeared to suffer comparatively little in the operation, which consumed about five minutes: not more than a gill of blood was lost.

The incision in the neck of the bladder healed by the first intention—not a drop of urine passed through the wound, after the patient was laid in bed. He was walking about the yard in ten days after the operation. He continued to discharge bloody urine, for four or five days, accompanied with some straining. After the bloody discharges ceased, the urine, after standing a short time, had a great proportion of very thick mucus, holding in solution a considerable quantity of a brick-dust like sediment.

The causes concerned, in the production of stone in the bladder, in the cases just related, are such as are but seldom connected with those diseases. Those who are acquainted with the anatomy of the human system and the pelvic region particularly, and with the œconomy of the system in relation to diseases of the bones and distribution of detached portions of bone, or other substances that cannot be destroyed by its vital action, (that may be taken into the stomach, or other parts of the œconomy, working their passage in various directions, performing a journey of several years, before arriving at the point of destination,) can without any extraordinary strain of inference, or credulity, easily satisfy themselves with regard to the manner in which these bones, in the cases before us, found their way into the badder. Some, however, have been willing to doubt the fact, or the possibility of bone getting into the bladder in the way I have pointed out.

I can see no necessity, why we should wander abroad into the wide ocean of conjecture, to conjure up a cause to ascribe

an effect to, when we have a cause of the most legitimate and efficient kind at home, under our immediate observation. In the latter case, the scale of bone in all probability, came from the ischiatic portion of the acetabulum, and possibly from the head of the femoris, passing through the thyroid foramen, making its way through muscles, ligaments, &c. into the bladder. Through what source the eyes became so singularly affected in this case, cannot be enquired into at present.

The disease of stone in the bladder, is becoming so common in this country, that it is thought the limestone water is the cause of it. There appears to be some foundation for this belief. I am of the opinion, however, that the manner of living and treating the common diseases of the country, particularly the diseases of children, have some agency in the production of this malady, independent of the kind of water made use of. It was suggested to me some short time since by Doctor Sansom, formerly of this place, who is a man of experience and observation, that blistering, carried to the extent that is fashionable here, particularly in treating the diseases of children, was calculated to bring on disorders of the urinary organs and stone in the bladder.

I operated on the son of a gentleman in this place, ten years old, for stone in the bladder, last November. When nine months old, he had a bad spell of cholera infantum, he was blistered very severely and had bloody discharges from the bladder, before he recovered. The symptoms of stone were dated from that period.

I operated on a little boy in Mount Pleasant, in December 1828, five years of age—his symptoms were dated from a spell of the same disease, at the same age. I did not learn whether the child had been blistered or not. There appears to be nothing wanting to give existence to stone in the bladder but a nucleus for the particles to adhere to.

When a calculous diathesis prevails, a nucleus that would otherwise be dissolved, would be more immediately covered over with calculous matter and terminate in a stone. So far as the general system is concerned, this disease, or rather the

avoiding or protracting its growth, may be somewhat under the control of medicine.

The case of Wallis afforded me an opportunity of observing the changes that took place, with regard to the comparative amount of calculous matter contained in the urine, at different times. While clear of fever and cheerful, there was but little sediment in the urine—when feverish and restless the wound was coated over with gritty particles, and the hair over which the urine flowed, looked like icicles. I presume the immediate cause of paroxysms of stone is generally owing to an increase of a calculous diathesis, when the rapid deposition of earthy matter renders the stone rougher and more irritating to the bladder. Could we control this diathesis, we would do much in alleviating the suffering of our patients, in those dreadful paroxysms when death itself would sometimes be a welcomed messenger.

ART. V.—*Annual Address for* 1830, *delivered before the Medical Society of the State of New York.* By T. ROMEYN BECK, M. D. President of the Society.

[NOTE. The following address, appended to the transactions of the Medical Society of the State of New York for the year 1830, which was politely sent us by the Author, contains within a short compass a very lucid sketch of the history of Small-pox, the introduction of Vaccination, and the arguments for and against its prophylactic powers.—From these considerations we are induced to transfer it to our pages, in as much as the public mind is at the present time particularly engaged on the subject.—EDS.]

GENTLEMEN OF THE SOCIETY,

WHEN I last met you in this place, I had hoped that the present session would have placed before you a member more worthy of your suffrages for its public offices. I was also aware, and I feel it peculiarly at this time, that the distinction of addressing you should not be continued long in one individual.

The range of subjects to be noticed is undoubtedly ample, yet their discussion by a single person may cause a sameness that will be tedious—and above all, he may at last fail, through repeated efforts, in producing that consideration, which we all desire should attach to our assemblage.

With a deep sense of these disabilities I have selected at this time, a theme which has little of novelty to attract attention, and which hardly permits of original suggestions. Yet it is one of permanent and abiding interest—not only to us as medical men, but to the whole community. When I mention that it is the SMALL-POX, its history and the possibility of its extinction, I should not exaggerate if I added, that it concerns the whole human race. The facts constituting its annals are among the most remarkable in the whole range of medical science, and they cannot be too frequenly impressed on the public mind.

It is now generally conceded, that the small-pox was unknown to the ancient Greeks and Romans. Their physicians have described the prevalent diseases in a manner sufficiently circumstantial, to be recognized at the present day and from their silence concerning a malady so very fatal, and so peculiarly characteristic, we have strong reasons to disbelieve its existence among them. The first medical account of it is given by the Arabians.

Rhazes, an accomplished scholar and physician of that country, who practised at Bagdad, in the beginning of the tenth century, is one of the oldest writers on the disease, whose works are now extant. He, however, quotes earlier authors, and from their narratives, it would appear that the small-pox was introduced into Egypt by the armies of Omar, the successor of Mahomet. The conquest of the Arabians would thus seem to have been the cause of the wide spread desolation of the disease; but they do not possess a sense as of the country in which it orig[illegible] [illegible] and [illegible] of its occur[illegible] [illegible] are discovered from their traditions and [illegible] [illegible] is so enveloped

in doubt, if not in fable, that no certain era of its commencement or its transmission can be fixed.

The facts, however, which these gleanings from ancient accounts furnish to us, correspond with the inductions of sound philosophy. In tropical countries, exposed to the heat of a burning sun, and suffering under the thousand evils of oriental despotism, it is evident that the human race must become subject to diseases of a peculiarly malignant type. The succession of ravaging armies, the destruction they brought upon conquered countries, the desolation they left within their own borders, the contamination of a fervid atmosphere, and the poverty of the lower classes, must have conspired to stamp fatality on their maladies; and accordingly in these regions, we find the countries of the plague and the small-pox. The latter, however, was armed with a double sting. Its contagious nature, propagating itself in every climate, has rendered the inhabitants of Greenland equally its victims with the "children of the sun."

We have hardly any authentic account of its introduction into Europe. It was at one time a prevalent opinion, that it had been imported by the crusaders, on their return from the Holy land. Their historians, however, take no notice of its occurrence in their armies, and it would appear more probable that it was brought into Spain about the eighth century, by the Saracens, and thus propagated over the other kingdoms of Europe. These followers of Mahomet, were at that time, a powerful and warlike nation. Their fleets were triumphant in the Mediterranean. They frequently invaded Italy. Rome itself was menaced, and they fought a bloody and desperate battle with Charles Martel, of France. Accordingly we find allusions to the prevalence of the disease in most of these countries, in the chronicles of the middle ages.

Emerging, however, from the uncertain light which the records of these periods afford us, we find melancholy traces of its progress in the sixteenth century. Columbus had just given to the world another continent, and his countrymen hurried to possess themselves of its treasures. They carried with them the subtle poison. St. Domingo, containing by computation, a mil-

lion of natives, was in a few years deprived of the whole number, by the combined effects of the sword and this dreadful epidemic. The magnificent empire of Mexico lost in a very short time, upwards of three millions of its inhabitants; and in several instances whole tribes of Indians, in both Americas, appear to have been nearly extinguished by its ravages.

In succeeding years, and in other countries, its path was no less marked by desolation. In 1707, an epidemic small-pox occurred in Iceland, and destroyed 16,000 persons, being more than one-fourth of its inhabitants. Greenland was attacked in 1733, and with such fatal acrimony, as almost to depopulate that sterile region. All the nations of Europe, and all the settlements made by Europeans in this country, have in turn been the victims of this disease, marked, according to circumstances, with a greater or less fatality. Indeed, wherever commercial enterprize has penetrated; wherever the sailor or the soldier has sought for conquest; wherever civilized man has visited, this dreadful malady has fastened, like the vulture of Prometheus, on the human race.

During the ages over which I have briefly glanced, the treatment was both arbitrary and fanciful. Experience was wanting, and observation was perverted. The patient, already labouring under a febrile disease, was harrassed by every means that could increase the heat of the system—warm air, an accumulation of bed clothes and heating remedies. And it is remarkable, that a peculiar colour was deemed important as a means of relief. Scarlet dresses for the patient, and scarlet coverings and hangings for the bed, were recommended by Avicenna, in Spain, and by John of Gaddesden, in England, as an effectual mode of drawing the blood to the surface and thus eliminating the morbid humours. The remedy was carried to Japan, where it acquired great reputation, and probably is still in vogue. And it is asserted that, even at the present day, it is yet in use in Portugal.

It was gradually, that better and wiser measures for relieving the symptoms of the disease, came to be introduced. A few physicians, from time to time, abandoned the indiscriminate and

excessive use of the hot regimen, and thus led the way for the great improvements of Sydenham. He allowed the suffering patient fresh air and lighter coverings, and interdicted the use of stimulating medicines. Viewing it as an inflammatory disease, he advises a corresponding course of remedies; and although he may have erred in some of his directions, yet the change he induced, materially tended to diminish the mortality of the disease. It remained, however, even in his hands, what it must continue to remain with physicians, not only of the present day, but of succeeding ages, a disease not to be cured, at least in the ordinary sense of that term. There are many maladies, whose progress the medical man can arrest, whose rapid effects he can instantly overcome; but he is comparatively a spectator with the small-pox. He cannot cure it; he cannot shorten its leading symptoms; he can only mitigate their violence and ward off their dangerous consequences.—From numerous data, deduced from the experience of various countries, it appears that small-pox proved fatal in one case out of every four or five, that contracted it naturally.

We arrive now at a very interesting era in the history of the disease. I mean the practice of *inoculation.* At first view, this would seem to be a most hazardous and daring experiment, voluntarily to introduce the matter causing so fatal a disease, into the human system, and accordingly we find that it was for many years in use among half civilized nations, before it awakened attention. It is probable indeed, that its power of communicating the malady, was a fortuitous discovery.

The most authentic accounts refer its origin to Eastern countries. In India and China, it appears to have been long known and practised, according to peculiar modes. It was introduced into Constantinople from the Morea, at a time when it was in general use, not only in Asiatic countries, but also in Barbary. It was stated to Dr. Russel, who resided long in Aleppo, that at Bagdad and Bassora, when the small-pox appeared in any district of these cities, it was the custom to give notice by a public crier, in order that those who were so inclined, might take the opportunity to have their children inoculated.

It was from Constantinople, however, and from a native of that city, a Greek physician, educated at Oxford, that the English nation and Europe first derived a competent knowledge of the advantages of inoculation. In 1714, Dr. Timoni communicated to the Royal Society of London, an account of the practice. He mentioned that the Circassians, Georgians, and other Asiatics, had introduced it for about forty years, among the Turks and others in that city; that although at first, the more prudent were very cautious in its use, yet the happy success it had been found to have in thousands of subjects, had now put it out of all suspicion and doubt. The operation had been performed upon persons of all ages, of each sex, and in the worst constitutions of the atmosphere, without any mortality following; while at the same time, it was very deadly when it seized patients in the common way. The matter was taken from some healthy person, on the 12th or 13th day of sickness, preserved in a close glass vessel, and when to be used it was mixed with the blood issuing from small wounds made with a needle in the arm, and inserted into them.

Other physicians continued their communications from Turkey; but the individual who more particularly attracted the attention of the British public to it, was a female, Lady Mary Wortley Montague, the wife of the English ambassador at the Ottoman court. Witnessing the success of the practice in numerous cases, she determined to have her own son inoculated, and the operation was accordingly performed by Mr. Maitland, surgeon to the embassy at Pera, in 1717. The consequent disease was very mild, and the patient very readily recovered.

On her return to England, she requested Mr. Maitland to inoculate another child, a daughter, which, after some demur, he consented to do. This was in 1722, and it was the first case in England. Several physicians witnessed the success of the experiment, and spoke favourably of it. The high rank and fashion of Lady Mary Montague, also aided in giving it notoriety throughout the kingdom. But it was still deemed a dangerous practice. At the instance, however, of the Princess Caroline of Wales, who had nearly lost one of her children by

the natural disease, and was very anxious to preserve the rest from so direful a malady, a more extensive trial was made. Six criminals under sentence of death, were offered the alternative, of suffering according to their sentence, or of submitting to inoculation. They embraced the latter, were inoculated by Mr. Maitland, and all passed favourably through the disease.

The impression made by these results, was heightened by information received from America. The small-pox, after an absence of nineteen years, had revisited New-England, and was very mortal in Boston. This had induced the Rev. Mr. Mather to reprint the accounts of Timoni and others concerning inoculation, given in the Philosophical Transactions.—The publication was distributed among the medical practitioners, and its perusal induced Dr. Boylston to commence the practice upon his own child, and afterwards throughout the place. In the course of six months, he inoculated 244 persons, but of these six died. So also in England. In a few months a mortal case occurred in an inoculated patient, and several servants attending on it, were all seized with the natural small-pox. Effects so contradictory to the assertions of Mr. Maitland, who had stated that the inoculated disease was uniformly mild at Constantinople, and who above all, had denied its infectious nature, were calculated to repress the rising partiality for it. A bitter and wordy controversy ensued. Inoculation was denounced by the press and from the pulpit, and its defenders were only listened to after the period of popular ferment had passed. Dr. Jurin, an eminent physician, examined the subject in a manner that all could understand. He looked into the bills of mortality for London, during a period of forty-two years, and also into accounts of deaths collected from other large cities, and he compared the numbers who died of the small-pox with the general mortality. From all these he deduced the facts, that of persons of all ages taken ill with the natural small-pox, there will die of that distemper, *one in five or six.* But from the inoculated disease, according to the experience both of England and America at that time, only *one case in fifty* had proved mortal.

An argument of such import, could not fail in time of produ-

cing its proper effect. The practice also extended in a favourable manner over Europe, and in various parts of our own continent. A hospital was established in London, under powerful patronage, for the inoculation of the poor, nor was it its least benefit, the temporary removal of patients from the community whom they might otherwise infect. Finally, two practitioners named Sutton, improved materially both the mode of communicating the infection, and of treating the disease. Inoculated small-pox was now deemed to prove fatal only in about one case in two hundred.

Such are the annals of this disease, down to the conclusion of the eighteenth century. Though its mortality was diminished, and its appearance no longer deemed the harbinger of death, yet it still proved destructive to thousands. In some countries the natural small-pox would occasionally break out with fierce and irrepressible energy. In others, even the inoculated would prove lamentably fatal. At the beginning of the eighteenth century, according to the bills of mortality for London, about one-fourteenth of the inhabitants died annually of the disease. During the last thirty years of the same period, although the practice had been greatly improved, and inoculation extensively pursued, the mortality had augmented to one-tenth. The loss of lives from 1770 to 1800, in Great Britain and Ireland alone, exceeded 35,000 *annually*. In other countries, the proportion was often greater, seldom less. Human skill seemed deprived of power to mitigate or diminish these melancholy results.

In 1798, however, a discovery was announced to the world, which promised some respite from the destroying scourge.—Dr. JENNER, a physician residing in a country town of England, presented an account of a disease, which in his opinion checked the influence of small-pox on the human system. He appears to have inquired into the subject, during the space of thirty years, and evidently pursued it in a spirit becoming the pupil of John Hunter. His experiments with the vaccine matter, were made in cases that admitted of full examination, and under circumstances calculated to decide the doubtful point which he presented to the public. But his discovery had scarcely been

announced, before others endeavoured to partake of the honor due solely to him. Their attempts to appear as the apostles of vaccination, ended in an exposure of their ignorance of its phenomena, and more unfortunately impeded its popularity and proper diffusion.

On a subject of such magnitude, it was not possible but that there should be diversity of opinion, and particularly among the medical profession. But it is one of the most enduring triumphs of the latter, that they, after due trial, by immense majorities, sanctioned its value and extended its use over every quarter of the globe. As members of the human family, they rejoice at the prospect of the extermination of a direful pestilence—as brethren of the profession, they joyfully abandoned the emoluments of one of its most lucrative parts, for the propagation of a disease (if disease it could be called) comparatively mild and needing but little of their attendance.

I will not occupy your time in tracing the history of the life of Dr. Jenner, which indeed is the history of vaccination. The obstacles he met with—the envy he encountered—the prejudices he overcame, and the final triumph of his efforts, are all too recent to need repetition. His country, partaking, in some points, of the character of our own, is occasionally tardy in awarding the meed of merit. But if there be any thing in this life worth living for, apart from the hopes of immortality, Dr. Jenner must have enjoyed it; and he has gone down to the grave with a reputation that, in the days of Greece and Rome, would have ranked him with their gods and heroes, and in our own, has placed him among that select and honoured few—THE BENEFACTORS OF MANKIND.

The impediments, however, to the complete and perfect diffusion of vaccination, and the consequent extinction of small-pox, have been numerous, and to a consideration of the principal of these, I propose devoting the remainder of this discourse.

1. The first I shall mention is the imperfect knowledge which unavoidably attends the promulgation of a new disease. Jenner studied it for thirty years, before he announced it to the public; but his experiments even then, though satisfactory as to their

result, were few in number. After vaccination became known to the British public, thousands supposed themselves equally competent with the discoverer to inoculate for it, and to decide whether the appearances and symptoms present constituted the real disease. They pursued their doubtful course without studying its characteristic marks—heaping mistake on mistake, and laying the seeds for subsequent and bitter disappointment. Others, we regret to add, took no precaution with regard to the matter used. In some instances, it was undoubtedly taken from persons labouring under small-pox, and thus produced that loathsome malady. The alternative was to confess their ignorance or to heap opprobrium on the new disease; and in some cases, there was no hesitation in embracing the latter. Even where this could not be charged, and were no suspicion could be entertained, either of ignorance or malevolence, there was unfortunately used a mixture of the variolous and vaccine matters. Again, during the period of enthusiasm consequent on an ill-timed and contemptible opposition, men out of the profession volunteered to practice the required inoculation. They supposed that nothing was necessary but to make an incision, insert the matter, and then the patient was protected. They were not aware of the circumstances indicative of its pervading the constitution, or of the peculiarities which characterise the external marks of the true disease. It is now well understood, that every interruption in the regular process of the local inflammation, may cause a failure in the cow-pox itself, and that it is imperative in most cases, if we wish to secure the individual, to re-vaccinate, or to repeat the insertion of the vaccine matter, during the progress of the current infection. Peculiarities of constitution are also to be noticed, as some resist its power in a most decided manner.

2. The influence of so subtle a contagion as that of the small-pox constantly pervading the atmosphere, and diffusing itself peculiarly among the crowded and indigent population of our large cities, has greatly tended to suspend the benign effects of vaccination. The former extends itself in an unseen manner. It is the arrow by night and the pestilence that walketh at noon

day. The cow-pox, on the other hand, must be communicated to every individual in succession—it does not propagate itself. Hence the contest must be unequal, and the result during the last twenty years has been the appearance of a disease which, with considerable propriety, has been termed the *varioloid.* It is not necessary to enter into its history, further, than to say, that a large proportion of the cases assigned to it, have undoubtedly been instances of natural small-pox—another part, the same disease, in persons imperfectly vaccinated, and therefore not secured—while some are unquestionably cases of a second attack of small-pox. That this does occur, and not unfrequently, has been proved by the observations of most modern practitioners. But subtracting all these, it would still seem that there are a few, but comparatively a very few, on whom vaccination has been performed with every precaution and with its proper results, and yet who during the prevalence of the contagion, have suffered under this modified small-pox. But the symptoms in these are much less severe, and the disease is shorn of its usual malignity. Even taking into consideration doubtful cases, it would appear from statistical observations, that under such circumstances, one fatal instance only, in every three hundred attacked, has been witnessed; and some indeed are disposed, on good grounds, greatly to increase this ratio. Can there be a more convincing proof of the efficacy of vaccination, if generally diffused?

3. And this brings me to another, but the most effective obstacle to the extinction of small-pox. It is the inattention—the indifference—the forgetfulness, of the community and of governments, to its character and consequences. When a nation enjoys for a few years exemption from it, a new generation springs up, most of whom have never been secured from its attack. The sky is cloudless—the mariner trusts to the winds to carry him safe, but he forgets that the monsoon may break up in a moment and overwhelm him in destruction. A case of natural small-pox is heard of in faint whispers. It has been brought by a vessel or by an individual from some distant place. He communicates the contagion—every person sickening, scat-

ters the poison wider and wider; and before we are aware of our danger, the plague threatens every dwelling. Vaccination is resorted to, in a hurried manner. The matter itself, in these cases, must be tarnished in its qualities through the all-pervading influence of the disease: yet we succeed in arresting the epidemic. A season of security follows, and time is again allowed for re-enacting the same scene some twenty years hence.

Can it be that our governments guard the lives of our citizens with sufficient care on this point? Let us observe what is done under despotic governments. In Denmark, by an edict issued in 1816, and addressed to all magistrates and bishops, it was ordered that all should be vaccinated; and without a compliance with this requisition, no individual could be received at confirmation, admitted into any school or public institution, or bound apprentice to any trade. The clergy were also forbidden to marry those who had not had either the small-pox or the cow-pox, and it was strictly enjoined, that in case of the former breaking out in any district, the proprietors should give immediate notice thereof to the proper authorities. The consequences of these peremptory measures have been as follows: During the twelve years preceding the practice of vaccination, 5,500 died of the small-pox in Copenhagen alone; whereas, since its introduction (1802) down to 1808, the deaths from it throughout the whole Danish dominions, were only 158.

In Prussia, the law forbids any one to approach a house in which the small-pox exists, and no variolous patient is allowed to expose himself abroad, until a medical man has certified that it can be done without injury to the public. All the costs of attendance are to be defrayed by the parent or master of a family through whose fault or negligence the disease has been introduced. And in order to encourage vaccination, the expenses of parents and their children, during the time they are passing through it, are defrayed by the state. Formerly 10,000 died annually in the Prussian dominions of small-pox. In 1817, only 2,940.

In Bavaria, by a royal edict, it was ordained that all persons above a certain age, who continued to neglect to be vaccin-

ated, should be fined by an increasing penalty every year, so long as they refused to take this means for their own protection. None were to be exempted from this fine, except they had previously laboured under one or other of the diseases—had been thrice vaccinated without effect—were below the prescribed age—or possessed a certificate from an authorized vaccinator, stating that ill health, or some disease in the system, made it proper to defer the operation. Houses in which variolous patients resided, were placed in quarantine for a month, and inoculation was forbidden under heavy penalties. In eleven years succeeding the promulgation of these laws, only five deaths happened throughout the kingdom from small-pox, and 162,000 were vaccinated.

In the kingdom of Wirtemberg, laws equally imperative with any that I have mentioned, are in force.

I do not pretend to recommend laws exactly similar to those that have been enumerated for our free governments, but I will say, that they furnish subjects for serious consideration. Whether some regulations could not be devised to arouse the apathy of that portion of the community, who are always the largest sufferers; whether the appointment by authority of medical men, particularly charged with the duty of vaccination, and preserving and transmitting the vaccine matter, and obliged to keep registers of those they attend; whether the promulgation of instructions, stating the dangers that threaten, the misery and mortality that may be avoided, the circumstances that prevent the complete influence of the cow-pox, and the precautions necessary for its constitutional effects; whether, in fine, a census should not be taken of those who have not laboured under one or other of these diseases, and they be compelled, under proper penalties, to submit to the latter—are suggestions, which to my mind, deserve some weight with those who have the power to render them imperative.

Life can be hazarded under our own roof, as much as in the field of battle, and the experience of all nations shows, that in this case, the chances have been fearful. When the means of prevention are within the power of a determined and united

community, what can prevent their adoption with as much efficacy as ever resulted from the mandates of an absolute monarch?

As a profession, we have not been wanting in sounding the alarm and providing against the danger. And it is a proud reflection, that the dangers of the small-pox, its wide spread ravages, and its constant succession, have been broken in upon by one who lived and died a physician. But he must be insensible to the loftier bearings of the subject, who can leave its consideration, without referring to the government of that Being, in whose hands are the "issues of all things."

Its history teaches us gratitude to that Providence which does not willingly afflict the children of men; which suffers physical as well as moral evil only for a season, and which, while it has permitted former generations to be scourged by ravaging infection, has, in mercy to us, removed the dreaded pestilence, or confined the operation of its destructive march.*

ART. VI.—*An Inaugural Dissertation on the effects of Emetics in Mercurial Salivation, submitted to the examination of the* REV. ALVA WOODS, D. D. *President, the Trustees and Medical Professors of Transylvania University, for the degree of Doctor of Medicine, on the 20th of February, A. D.* 1830. By THOMAS CALHOUN NELSON, of Decatur, Alabama.

HOW long mercury has been known to the medical world as a most important article of the materia medica, its history does not enable us to say, precisely. We have reason to believe that it was not in general use, as an internal remedy, till towards the close of the fifteenth century; about the period of time at which

* The works that have been particularly consulted, are, Woodville on Inoculation; James Moore on the Small-Pox and Vaccination; Munro (Tertius) on the Small-Pox; Thompson and Cross on the Varioloid, &c.

the syphilis began to attract such general attention throughout all the South of Europe.

We are told by its historians, that, by the Greeks and Romans, anciently, it was regarded as a most deadly poison; and hence excluded from their very imperfect materia medica. Its first application to the treatment of diseases, was by the Arabians, as an external application in cutaneous affections: subsequently, by Oriental practitioners, taking the hint from its effects in cutaneous eruptions and glandular enlargements, it was prescribed in plague, though, perhaps at first only as a remedy for the local affections, such as buboes, &c. attendant on that disease. Used in such cases freely, it would as a natural consequence, not unfrequently produce its specific effect on the patient, and this specific influence on the system, would often greatly palliate the disease, or perhaps, happily relieve it entirely. Hence we may fairly account for its application to the treatment of all diseases bearing any analogy to the plague.

Apprized, from its use in these cases, of the controlling influence which it exercises over febrile affections, eruptions, buboes, ulcers, &c., it would most naturally glide into the class of antivenereals, and as naturally, take its stand at the head of this class of remedies.

We are, moreover, informed that it was most extensively employed as an external application, by the Europeans, long before the syphilitic disease attracted the attention, or excited the consternation of the medical, as well as the balance of the civilized world. An article so active could not have been in common use, without many of its virtues becoming familiar to all who had any great degree of tact at observation or reasoning.

Accident too, must have admonished many of its powers and its virtues, in ages very remote from the present. Regarded anciently as a violent poison, it was, doubtless, often resorted to for sinister purposes, by the evil disposed. Taken under such circumstances the devoted individual would often, instead of being hurled from the stage of action, derive most salutary and obvious relief, from its medical influence over diseases, under which he may have been, for the time, labouring.

But let its manner of introduction into practice, and the discovery of its virtues, have been what they may, certain it is, that its unparallelled efficacy in the management of a large majority of the diseases to which the human race is liable, is now most generally conceded.

In almost all the diseases of the circulatory, the secernent, and the chylopoietic apparatus, in some form or other, it is constantly prescribed, and constitutes the "main anchor of hope." In some of the diseases of the system, as hepatic and syphilitic affections, it is, if not a specific, almost absolutely necessary to their successful treatment.

In syphilis, it has been denied, by many eminent writers and practitioners of the day, especially those who deny the specific nature of its poison, that mercury is necessary to successful treatment. But from the experience of others, not at all their inferiors in any respect, we are compelled to say, that the merely antiphlogistic plan, will not succeed in all cases, if it will in any. We do not propose entering upon this contested and interesting subject in this paper; namely, whether the poison be specific or not, and whether the antiphlogistic plan of treatment is alone sufficient. But, as a reason for believing on the subject as we do, and as it may not be entirely void of interest or bearing on the subject before us, we will take the liberty of presenting the following case, in as concise a manner as possible.

Mr. M. a gentleman of irregular habits, but of firm constitution, aged about 25, applied, July 15th, for advice in a case of gonorrhœa. He said that three weeks previously he had cohabited with a female of the town, from whom he contracted the disease, which made its appearance a few days afterwards. He upbraided her for having imposed upon him, (for she had assured him, at the time of their intercourse, that she was free from disease,) when she acknowledged that she had had the disease sometime previously, but that she had thought herself cured. Shortly after their intercourse, however, it reappeared on her.

The local inflammation was considerable at the time of his application for relief; for being anxious to get well before the

time of an approaching ball, he had taken the bal. cop. in quantities as large as his stomach would bear, and used injections of a most stimulating nature.

He was plethoric and his tongue slightly furred, though he complained of no chylopoietic derangement. He was advised to lay aside the washes and bal. cop. for the present, and reduce his system and correct its febrile tendency, by free venesection and cathartics of a mercurial and saline character. In the course of a few days the disease had so far subsided, that he considered himself as cured; but from free indulgence in eating, drinking and dancing, it reappeared with all its former violence and required a renewal of the same plan of treatment, persevered in for ten or fifteen days, to relieve him again.

Before he was entirely relieved of the most violent symptoms of the disease, he applied for relief in a disease of a different character. Having imprudently indulged his propensities with a female, of a similar character with the above, he was mortified, a few days afterwards, by observing all the symptoms of the pox, make their appearance.

August 10th. He exhibited venereal ulcers on the prepuce and glans penis; and shortly afterwards, others made their appearance on different parts of the genital organs. He immediately commenced a mercurial course, and continued it, though not very actively, till the 22nd, when he rode eight or ten miles from town, during an excessively warm day. The next day he was attacked violently with a bilious fever, which prevailed epidemically at that time. He became the patient of a physician, who treated him actively for the fever, without being apprized of the other complaints; for both pox and gonorrhœa still continued on him at the time he left town.

He was bled, took emetics and cathartics freely at the onset of the fever for several days. Three days after the access of the fever, the gonorrhœa disappeared, and in eight or ten days the venereal ulcers all healed perfectly.

After he had been treated for the fever, by active remedies, till he was considerably reduced, febrifuge remedies, bark, diet. &c. only were prescribed.

It was September 8th before he was able to leave his bed, and before he was entirely relieved of fever.

Sept. 12th, the urethral discharge again made its appearance. Not very long afterwards, buboes appeared in his groins, and copper-coloured blotches on different parts of his body.

Sept. 20th, he returned to town, resumed a mercurial course, and continued it actively, till about the 25th of October, when he was thought to be cured of syphilis. But the urethral discharge still continued through all the courses of syphilis and mercurial salivation. And it was not till after the strictest attention to diet &c., with the constant use of balsam and astringent injections for several weeks longer, that he was relieved of this troublesome disease.

The above case is interesting on several accounts. The gonorrhœa was first contracted, a local specific disease. While yet labouring under this disease, the syphilitic infection, another specific disease, was received into the system. They continued in the system, though located perhaps in different tissues, moving *pari passu*, for several weeks; and before either of the diseases was removed, a third made its appearance, involving the entire system. The bilious fever had not continued more than a few days, until the former diseases became suspended, and attracted no further notice from the patient till after he was entirely relieved of fever. But they were only suspended, not expelled. So soon as the more general disease disappeared, the two former diseases became again conspicuous. And from the manner in which the syphilis was first observed, after the removal of the fever, it would seem that it had not at all been dormant, although its progress was unnoticed during the continuance of the bilious fever; for the disease at first local, was evidently now a disease of the entire system.

But the circumstance to which we, at present, wish more particularly to direct attention, in this case, is the evidence afforded by it, that mercury is necessary to the treatment of syphilis. Its use was at first commenced, but from the attack of fever and change of attendants, it was dropped before it had been continued long enough to cure the disease. An actively antiphlo-

gistic course was then pursued for nearly a month, for the relief of the fever, but with no other effect on the syphilitic disease than that of healing the chancres; for the disease was not relieved, nor do we believe that it was much if at all arrested by the treatment. Almost at the very moment of the cessation of the fever and the cessation of the plan of treatment adopted for its relief, the venereal disease again attracts our attention in a constitutional form, and finally required a full course of mercury for its entire relief. Other particulars in this case, and many other cases of a similar character, tending to urge belief in the specific nature of the venereal poison, and the necessity of mercury in some form for its relief, might be detailed were it necessary; but as this is not requisite for our purpose, we think the above, on this point, sufficient.

In the varieties of hepatitis, chronic and acute, mercurial remedies are almost as celebrated and as confidently resorted to, as in the syphilitic affections. In several of the hydropic diseases, in nearly all the deranged conditions of the chylopoietic viscera, and in fact in nearly every disease to which the system is liable, mercury in some form, or some combination, makes up a conspicuous part of the prescription.

If cautiously administered, there are few diseases, if any, in which it would not prove decidedly beneficial. Observation and experience render a proof of this assertion, by examples detailed, unnecessary at this enlightened period of medical science.

Although the impression made by mercury is, in many respects, very mild, yet there is no medicine with which we are acquainted that produces effects, at the same time so universally diffused, so permanent and so powerful. Hence its deserved repute in the treatment of diseases.

As a cathartic alone, it is invaluable, on account of the direct and powerful effect which it has over the liver, the largest viscus in the system, and one whose influence is more widely extended, and whose functions are more frequently, and more deeply involved in disease, either directly or indirectly, than those of any other organ of the body.

In all febrile affections, in all pulmonary diseases, in every form of chylopoietic derangement, this gland is at one stage or other involved to a greater or less extent. And, in very many diseases we can afford most signal relief, or even effect a complete cure, by directing our remedies to the liver, even though that organ may not have been primarily affected.

If mercury, then, had no other quality than its cathartic effect, we could but imperfectly succeed in the treatment of diseases without it. On account of this property alone, we would prefer it to any one, or even to all the articles of the Materia Medica besides.

But it possesses other valuable qualities, that, not less than its cathartic virtues, recommend it most strongly to our notice. We can scarcely imagine a property that it does not possess, or an effect that may not be produced by it, in some degree or other, if judiciously managed in some of its forms.

But that property for which it has been principally celebrated and to which we design principally directing our attention for the present, is its specific effects on the system,—its powers as a sialagogue. It is on account of this quality, that it has gained so much renown and that so much odium has been cast upon it. This is a property most valuable, and one on account of which we would less readily dispense with this article than with any other of the Materia Medica; for with no other article at this time in common use, can we produce this constitutional effect, so necessary to the successful management of many diseases.

In many diseases in which mercury as a cathartic is, if not indispensable, at least a medicine preferable to any other, its peculiar action on the constitution; is such as to prevent its use for the careless and free use of it is attended with such consequences as to render it extremely dangerous and unpopular. He, then, who shall convince mankind that the constitutional effects of mercury need not, necessarily, be so painful and so dangerous, and need not always prove a consequence of its free and protracted use, as a cathartic, will go far to remove that dread, and that odium entertained by many towards it; and he who

shall favour the medical world with the certain means of preventing these effects, or of removing them promptly when produced, will confer a favour of some magnitude on mankind. There are few who have seen mercury administered freely in the treatment of disease, who have not witnessed its melancholy ravages on the constitution, and who are not able to detail a list of fatal cases resulting from its use. So long as these cases shall be of as frequent occurrence as they are at present, this valuable article will either be disused by the cautious and the feeling, or prescribed with such diffidence and anxiety as to render it comparatively ineffectual.

It is for the purpose of calling the attention of the Faculty to what may prove a palliative, if not a valuable remedy in such cases, that this paper is designed. We have not commenced the task without a full sense of the magnitude of the undertaking, nor are we at all unaware of the doubts that may arise as to its efficacy in all cases. But our remarks may suggest to others what is so much to be desired—means less doubtful and more efficacious. We cannot do more in this imperfect article than point the attention of others to the hint which was first received from one of the able teachers in this University, and afterwards deeply impressed on our mind by the observance of an accidental circumstance, which we beg leave to relate, with other cases that may be necessary to exhibit in as strong a point of view as possible the claims of the plan of treatment we propose to the consideration of the Faculty.

The first circumstance which called to mind the lectures we had heard in this school on the subject of the controlling influence which emetics exercise over the different tissues of the system, was the observation of the following case.

Oct. 15th, 1828. Mrs. B——was attacked with fever of the remittent form. In the evening she took a large dose of calomel. 16th. The medicine had not operated: the disease continuing to progress, advice was solicited in her case late in the evening. When visited for the first time her symptoms were violent, the intellect deranged, skin hot and dry, pulse strong and frequent, eyes dusky and suffused with blood, respiration difficult

and laborious, tongue deeply furred, stomach irritable, bowels obstinately constipated. She was freely bled, so much so as to produce a strong impression on the system. Twenty-five grains of calomel were given with directions for her to have ol. ricini, ℥i. two hours afterwards (at 7o'clock.) At 12 P. M. reaction having come on after bleeding, and no action on the bowels from the medicine, no abatement in the symptoms, stomach still very irritable, calomel, aloes and rhubarb, of each 15 grs. were given: the medicine was retained with difficulty.

17th. In the morning; no operation from the medicine. Castor-oil was again resorted to with injections. During the last night, she complained of her mouth being sore and swollen. This morning it was still more affected and the flow of saliva was very considerable. In a few hours the medicine commenced operating; and by 2 o'clock three or four tolerably large, consistent discharges had taken place. There was now some abatement in the general symptoms, but the ptyalism and swelling of the gums were considerably increased, and the tongue had become quite tumid. She was directed to take a solution of tartar emetic (4 grs. in half a pint of water) in small quantities every half hour till it produced an abatement of the fever, or produced vomiting. At 4 o'clock it produced free emesis; and the nurse mistaking the directions, continued to administer it till 8 o'clock, at which time she was again visited. She had vomited freely and frequently during the evening. There was an abatement in the febrile symptoms; the skin had become cool, and the tongue more clean. No further operation from the medicine, no complaint whatever of the mouth, though the saliva flowed as abundantly as ever. But there was evidently less swelling of the gums than at 2 o'clock.

This circumstance particularly attracted our attention; for we had calculated certainly that the salivation would continue to progress for several days. Being unable to account for the sudden and unlooked for abatement, from any other cause than the effect of the tartar emetic, we determined to repeat it the next day, and to continue to do so till it had effected an entire cure, if it possessed the power of doing so. She took tartar

emetic enough on the 18th (at which time little change was noticed in her condition from the day previous) to operate mildly four or five times during the day, with very evident advantage to her mouth.

The next morning she was so entirely relieved as to render it unnecessary to repeat the emetics.

From the occurrences in this case, and from a recollection of the lectures of the Professor of Surgery in this school, by which we had been taught the great value of emetics in diseases of the stomach—of the mucous membranes generally—of the glands &c. we were irresistibly led to the belief, that the prompt cessation of the constitutional effect of mercury here, was owing to the free emesis produced by the tartar emetic, which had not been prescribed for this purpose, but merely for the sake of relaxing the system, reducing the febrile action, and of assisting the action of the cathartics previously administered. Such was our conviction on the subject, that we determined to give the remedy a fair trial so soon as an opportunity should occur.

Some time previous to the occurrence of the above case, a small boy, 8 or 10 years of age, was visited in company with two medical friends. He had been so deeply salivated by the calomel he had taken in a recent attack of fever, that death appeared inevitable. His gums, tongue, and entire lining of the mouth, were black and beginning to slough, there were several ulcers of considerable size on different parts of his tongue, lips, and gums. In some places the gums had fallen from the inferior maxilla, leaving it entirely bare. Those of his teeth that had not already fallen out, were so loose that they might have been easily removed with the fingers. Very little flow of saliva, pulse rather small and frequent than otherwise, bowels constipated.

The usual plan of treatment in such cases was resorted to: Flor. sulph. freely as a laxative; opium and ipecac. as a diaphoretic; cold water acidulated with elix. vit. as a drink; poultices of carrots, charcoal, barks, &c. and the usual washes of tinct. myrrh. &c. His tongue, the integuments of his face and finally a part of the lower jaw sloughed away. He died.

This case is presented for the sake of comparing it with the following one, and of contrasting their treatment and event. That any plan could have saved the patient we will not say: his case may have been too desperate from the time he was first seen, to have been successfully treated by any remedies. An emetic might have killed him at once. It would, to say the least, have been a very hazardous practice. But it would have shortened his existence but a few days; and it might have saved him.

Oct. 29th. A case not very unlike the one last detailed presented itself, in which the emetic plan was used with the happiest effect.

A young female 12 or 13 years of age, from a most careless kind of treatment, and a most incautious use of calomel, was reduced to a condition almost as desperate, as that of the little patient above mentioned. When we saw her, she had some excitement of the arterial system, though it was not very considerable; her pulse was frequent but small; her countenance cadaverous; her tongue, gums, the lining of her mouth &c. swollen and dark; flow of saliva small, teeth dark and so loose that they might have been shaken from their sockets. At a point on the upper jaw, at which there was a decayed tooth, there was a considerable slough, and ulcers were in different parts of the mouth. Her bowels were constipated at the time, though they had been previously lax.

We pointed out the danger of her situation to her parents; we related the case of the boy above referred to, its treatment, and termination. We also related the case of Mrs. B. and pointed out the probable hazard of the plan we would advise, as the only one we could with confidence rely upon, in the young lady's case. They agreed to risk the consequences.

We accordingly administered an emetic of ipecacuanha, fearing the prostrating effects of tart. emet. in her reduced condition; for she had been labouring for two weeks under an attack of fever. The emetic operated well; afterwards she took a gentle cathartic of rhubarb and magnesia, with a mild enema. In the evening, observing no unfavourable effect from the emetic, it was repeated, and operated freely, though mildly.

On the 30th her gums &c. were more florid, and the flow of saliva had increased considerably. She took three emetics, through the day and fore-part of the night, with evident advantage.

The gums on the morning of the 1st Nov. were not so much swollen as the day previously. The ulcers all looked healthy; the flow of saliva was free. She took three emetics during this day and night, and on the 2nd every symptom was so favourable, that a repetition of the treatment was not thought necessary.

In three days from this time, she ceased to complain of her mouth entirely, except at that point of the jaw in which was the carious tooth; from which came very small scales of bone, occasionally, for several months.

What can be more satisfactory and convincing than this case? What charm could have acted more promptly? Every symptom would have taught us, that the case, so much like the one of the little boy, must terminate as that did; for the circumstances as well as the symptoms attending the cases were very similar. But a change, as favourable as it was desirable, took place immediately after the treatment was commenced.

It may be said, that the cathartic should be regarded as a principal cause of the happy effects of the plan of treatment. But cathartics will not alone succeed in such a case, as was fairly proven by the previous case of the boy; and numerous cases of a similar character might be adduced, in which the cathartic plan has been pursued to various extents, has been fairly tried, and fairly failed. It is uniformly unsuccessful. The catharsis in this case was not very considerable, nor could it have produced so prompt an effect under any circumstances. But we need not dwell longer on this case, for its attendant circumstances and termination are sufficient commendation.

August 10th, 1829. Mrs. G. aged 38, about three months advanced in pregnancy, was attacked with congestive bilious fever. The chill was severe and protracted, the pain in the extremities, back and loins was most torturing: added to this were pain and cramp of the stomach of so alarming a kind as to threaten a speedy period to her existence. She vomited, or

stand forward to our view in bold relief, as the most powerful and efficacious remedies in all cases of salivation, in which they can at all be borne.

When the system is in such a condition that mercury produces but imperfect effects on it, administer an emetic, and the system is at once unlocked, its powers are relaxed, and its susceptibilities are rendered alive to its impressions. When the system is in too irritable a condition, when its susceptibilities are too great to admit of the exhibition of mercury, without dangerous consequences, emetics temper it down admirably, and exercise a finely controlling influence over it.

That what are called idiosyncrasies of constitution do exist, forbidding the use of mercury in any form, we do not deny; but we have little doubt that if emetics were resorted to under such circumstances, combined with mercurials or alternated with them, these idiosyncrasis would be greatly less frequent, if found at all. We have never yet had the opportunity of testing this opinion by experience, and therefore, can only present it as an opinion.

Idiosyncrasies of an opposite character are said to exist also; individuals upon whom mercury will not, under any circumstances, have any other than its cathartic effect. Numerous cases of this kind may, with little trouble, be collected, but these if rightly tested, we believe would be as scarce as those of the opposite kind. Many of them we are certain might be removed by the use of emetics.

Mr. M. T. affected with syphilis, was three months under the treatment of a physician, who used every form of mercury at one time or other, under every form of administration, for the purpose of producing the specific effect of mercury on the system, but he never succeeded; and the young gentleman applied to us for relief, saying that mercury could not salivate him, that it had been frequently attempted previously, but always without effect. We gave him an emetic of tart. ant. every morning for five days, followed with 20 grs. of calomel, shortly after it had ceased to operate. At the end of this time we commenced the use of the blue pill, in such doses as not to act

From some cause or other, either from the effect of the narcotic, the affusion of the cold water, or the action of the emetic, the cathartics which had lain in the stomach, without producing any very considerable effect previously, now received a new impulse, and produced during the day (12th) several very copious and consistent bilious discharges. The prospect for a speedy and entire recovery was now very flattering.

She had very trifling febrile excitement during the day, no return of the chill, no symptoms of colic. But towards evening she began to complain most bitterly of her mouth, and upon examination next morning (13th) her tongue was found tumid, her breath fetid and of the peculiar odour that mercury imparts; her gums of a dark purple, and studded over, in different parts, with small ulcerations; the flow of saliva was considerable; the sense of heat and burning in the mouth so considerable that the complaints of the patient were most incessant. From the amount of mercury taken by the patient, and the length of time it had lain in her, with but slight sensible effect, we dreaded the extent to which the salivation might extend, if not speedily arrested. We immediately had recourse to emetics. She took two during this day, but not with very decided effect, for they operated but imperfectly. During the ensuing day, she took three emetics of tart. ant. which operated much better than those of the previous day.

On the morning of the 15th her mouth was very decidedly on the mend; so much so indeed that it was with difficulty we could prevail on her to repeat the emetic. The swelling and ulceration of the mouth had almost subsided, the only remaining effects of the mercury being some redness of the gums and a free flow of saliva. She took but one emetic during this day, and on the 16th there were no troublesome or painful effects of the mercury remaining.

Many other cases of a similar nature might be detailed, exhibiting the certain and prompt effect of emetics in the relief of salivation, in a strong point of view; but these, we think are sufficient to establish their claims to the attention of the medical profession. We cannot but see from the above cases that they

are effects which have been noticed by those, who have witnessed the exhibition of mercury to any considerable extent.

Let us contrast the effects of emetics. The first visible effect of an emetic, especially of the antimonial class, is a decreased action of the circulatory system. The action of the heart is languid, the pulse weak and fluttering, the countenance pale and languid, the surface and extremities cold, the skin relaxed and covered with a clammy perspiration. When the nausea becomes considerable, the action of the heart becomes still more weakened. The energy of the nervous system and of the capillaries, is reduced, so that a free discharge of all the secreted fluids takes place. No remedy, with which we are acquainted, so completely allays the irritability of the system, as emetics.

Thus from the directly opposite effects produced by these agents, we could very confidently expect the highest degree of relief, afforded in the promptest manner possible, from the action of an emetic in a case of mercurial salivation, without once having witnessed its exhibition. And this we have seen from the cases above detailed, is most certainly and evidently the case.

ART. VII.—*On the remote and exciting causes of Autumnal Fevers.* By Dr. CHARLES LEE PAYNE, of Lexington, North Carolina.

NEXT in importance, in a practical point of view, to the pathological phenomena that present themselves in diseased action, are the causes which have predisposed, led on, and ultimately produced these effects. Post mortem examinations have greatly contributed to the acquisition of the former; and many facilities are afforded to the American physician for the accomplishment of the latter. Of all the diseases that affect suffering humanity, fevers have excited the greatest attention of medical men: nor should we be surprised at this, when we consider what an immense avenue it opens to the outlets of human ex-

istence, and how nearly allied they are to the king of terrors itself. Any attempt therefore, at coming to correct and rational conclusions upon the causes of fever deserve the attentive consideration of every physician.

Various and numerous have been the causes assigned as the agents productive of autumnal fevers; but medical opinion seems now, and has been for ages past preponderating in favour of marsh effluvia as the most fruitful source of fevers: indeed most of the learned and eminent physicians of every age since this theory of Lancisi was first promulgated, have been decidedly of the opinion of its miasmatic origin. Some however have ventured of late, to dissent from this far famed and long established opinion, and we may by a careful examination of the writings of the ancient as well as modern physicians, see plainly their doubts upon the permanency and truth of this assumed hypothesis. Sydenham resorted at times to a peculiar and unaccountable constitution of the atmosphere as the cause of epidemic fevers.

A forcible array of names and talents have resorted to contagion. Carbonic acid gas, nitrogen gas, heat, and moisture, have respectively inspired the pen of the speculative and ingenious; which evidently show a combination of testimony in favour of other causes; and doubts as to the commonly and almost universally received opinion, marsh-effluvia.

It has generally been admitted, that animal putrefaction has never exerted its deleterious influence so far as to produce fevers: vegeto-animal putrefaction has also been considered equally unproductive of epidemic fevers; but this unaccountable "something nothing" marsh effluvia, has been spreading devastation and terror, and peopling the regions of the dead with more than half her votaries ever since the days of Lancisi, or marsh effluvia could inspire a writer in her cause.

Reasoning by induction I would frankly acknowledge that there is some good ground for this long and generally received opinion. We all now know that marshy, flat, and badly drained districts of country are sickly; that epidemic fevers prevail there most, and the more particularly when there is that due

mixture of heat and moisture necessary for carrying on the process of putrefaction: but does this prove that vegetable putrefaction is the universal cause of epidemic fevers?

Reasoning by the same rule of induction then, the epidemic would not only be more general, but the cases most violent and unmanageable. That cases are more general in districts of country most favourable for carrying on the process of vegetable putrefaction is most certainly true; but this I will presently make an effort to shew, may be owing to other equally appreciable and more tangible causes.

That cases are most violent in districts of country favourable for carrying on the process of vegetable putrefaction, cannot be proven: my limited experience teaches me otherwise, and I have but little doubt that universal experience will coincide with me in this opinion: the most unmanageable cases, that I have seen, have been in comparatively elevated situations, sporadic cases. Our mountains and hill country can respond to the truth of this assertion, and it has almost become proverbial among practitioners inhabiting low and marshy countries, that their cases of fever though numerous are generally manageable under judicious and well directed practice. Cannot equally as good reasons be produced to prove that moisture coming in contact with an exhausted, fatigued, relaxed and debilitated constitution, may be as general a cause of fevers as marsh effluvia? And when this cause is preceded by the relaxing, exhausting, and debilitating remoter causes excessive heat: are not the two combined, sufficient in themselves to produce fever? This in the humble opinion of the writer is the bugbear that has annoyed us for ages past, and that no age will ever be exempt from, and that no foresight or knowledge can eradicate.

Whether that peculiar gas or something extricated by the decomposition of vegetable substances has ever produced one case of fever is extremely doubtful. It appears to me much more rational and consistent with our present pathological views of the operation of deleterious agents on the human system to consider as the general cause of epidemic fevers, the compound influence of heat and moisture.

The effects of free caloric upon the human frame is obvious to all, its tendency to expand all bodies in nature is a truth that requires no comment; and when this agent is exerted by the scorching rays of a vertical sun, 'tis then we see in the most eminent degree, its expanding, relaxing, and debilitating influence on the muscular fibres which constitute so great a portion of the human structure. From the effects of heat on the human system (which is much increased by exercise during the heat of the day in those fertile countries where fevers most prevail) I am disposed to place it with Dr. Collins of Louisiana as the remote cause of fevers. I have long entertained this opinion, but would add another essential and more proximate cause, *moisture*, as equally necessary in the production of autumnal or epidemic fevers.

Heat, preceding moisture, appears to give to the whole animal system, that peculiar fitness and aptitude for the operation of the second cause, moisture, that I feel confident its baneful influence could not otherwise be exerted.

Take a thousand soldiers relaxed, fatigued, and exhausted by a forced march under the heat of a vertical sun; plunge them in this situation in a cold bath; imagine to yourself what would be their situation; particularly if this process was repeated for weeks at a time, and they continued in the bath some hours each time: would not this sudden transition produce Cullen's spasm of the extreme vessels, or the cold stage of intermittents, or the chill of remittent bilious fever? And is not this measurably the case with the southern planter, who has under his charge numerous slaves in the cotton fields, with drivers to keep them actively engaged during the excessive heat of the day? leaving them fatigued, relaxed and exhausted by the double operation of heat and exercise? And are they not at night exactly in that situation most suitable for the operation of the second cause, moisture, which owing to the heat of the day has been evaporated from mill ponds, swamps, and marshes, and again condensed by the change of temperature at night, into a partial cold bath.

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heat and moisture as being of themselves the most active agents in the production of fevers.

Heat promotes perspiration, unlocks the secretions from the skin, relaxes and expands the exhalent orifices: the evening coming on perspiration is checked and the immense mass of matter which during the day passes off through the emunctories of the skin by this change, moisture, is driven in upon the internal organs producing dysentery, cholera(or Sydenham's fever turned in upon the bowels,) intermittent and remittent bilious fevers. The lungs, stomach, and all the other viscera are frequently affected; but owing to the thinness of the parietes of the abdomen and its being defended by no bony structure; it is oftener the seat of its first noxious, and debilitating influence.

Sudden vicissitudes or changes of temperature, we willingly acknowledge to be the active agent in the production of many diseases: pneumonia and many of the phlegmasia are attributable to no other causes. When changes of temperature take place, if these changes go on gradually, the animal economy becomes accomodated or adapted to them, so that no injury or but partial is sustained: but whenever these changes are very sudden, as certain will these changes be shewn by a group of diseases denominated phlegmasiæ.

I have designedly placed heat and moisture as the most active agents in the production of autumnal epidemics, but cold combined with moisture is also an essential ingredient in the list of causes. These three independent of any other causes are sufficient of themselves to produce more cases of fever than it is possible for all the votaries of marsh effluvia to demonstrate can be affected by that agent. We are all (cæteris paribus) aware that in proportion to the heat of the sun will be the quantity of water evaporated during the day: this being suspended in the atmosphere, expanded and rendered light by the pervading influence of free caloric is no longer cognizable to the senses, until the change which is produced by the cold of night; when this evaporated fluid is condensed, and by its gravity falls nearer and nearer the surface of the earth until it ultimately

reaches it, assuming the common name of dew. In this place I would remark that the supporters of marsh effluvia, contend that it never intrudes upon upper stories or elevated stations. Heat, moisture, improper regimen, passions of the mind and many other causes, have been assigned by the supporters of marsh effluvia, as, the exciting agents of fever; but say they the predisposition was merely the foul miasm of a marsh, mill pond, or some other place where vegetable matter had accumulated and putrified, and this, after producing its peculiar and noxious impression, remains partly dormant in the system until some of the above exciting causes, (perhaps six months afterward) rouse, awaken, and develope its peculiar and specific operations.

This would appear to me to be strange doctrine! Why do not the advocates of contagion in typhus and yellow fever resort to the same hypothesis?

The supporters of marsh effluvia certainly believe that it is a more powerful agent than contagion; and yet they have to resort to other helps to bring it into active exercise. It will be admitted without controversy that most of the phlegmasiæ derive their origin from sudden changes in atmospheric temperature: in all these fever is developed and progresses with the violence of the local affection: if sudden changes from heat to cold in February or March can produce pneumonia characterized by a fever which seldom leaves the patient until the ninth day or later; is it not rational to conclude that in August, September or October, when the heat of the day is violent and the nights sufficiently cold to condense into a fluid state what had before existed only in vapour, this last acting upon the exhausted, lax and debilitated fibres so characteristic of warm climates, may produce fever?

It was a maxim or aphorism of Sydenham that during the prevalence of epidemic fevers, all minor diseases partook of the nature of the prevailing epidemic: this cannot be doubted: hence, we have pneumonia biliosa, and many other of the phlegmasiæ shewing themselves during the winter under a marked or complicated form.

The champions of miasmata would take this as additional

proof of their favourite theory; but cannot this phenomenon be accounted for upon more rational and pathological principles? It is in these they would say that the impressions made by the effluvia have remained dormant, one, two, three, six and perhaps nine months, until it is excited into action by other agents or causes: but may we not conclude that the debilitating influence of heat in various degrees, with other causes before assigned may so modify and operate upon the constitution as to produce these phenomena? The fertility of soil in marshy districts of country, present many inducements to the enterprizing and industrious, for active exertions. In South Carolina, Georgia and Alabama, the cotton making business exposes its thousands, and in Lousiana, Mississippi and the Indies the manufacture of sugar has so exposed those engaged in it to the scorching rays of the sun, that it too has contributed in an eminent degree in peopling the regions of the dead with victims of fever.

All who have any experience in fevers must admit that these causes, independent of marsh effluvia, can produce fever: much less active agents have produced it: who has not seen fever produced by active exercise for a short time in hot weather, carried to exhaustion followed by some imprudent act, such as bathing the feet, head, or neck in cold water, or even sitting in cool air, and this fever showing too all the marks of the prevailing epidemic? but in all these the impression had previously been made, but the miasm had not developed itself into fever. Now all this I consider equally as hypothetical as Sydenham's peculiar constitution of the atmosphere.

Since the days of Lancisi, who, perhaps was the first author who wrote on marsh effluvia as the cause of fevers, the medical world have been gradually yielding to this doctrine: some however in every age, have been led to doubts on the subject—hence contagion has been a prolific theme of controversy.

What medical man does not recollect the collision excited towards the close of the last century on this subject, when the yellow fever made its appearance in Philadelphia. The philanthropic Rush was then almost alone supporting and defending the cause of marsh effluvia. A galaxy of medical names

and of talents was inlisted against him and rendered for a time his favorite theory unpopular. He however outlived all opposition, and his doctrines became by and by standard and orthodox. But does all this prove or establish the fact that even yellow fever derived its origin from the putrefaction of the filth of a city or from marsh effluvia? Or has the experience of the best informed decided the question and convinced themselves and the world, that it took its birth received its origin, and spread with such rapidity from contagion?

"The candid upon either side must acknowledge that clouds and darkness rest upon it." "How then will I hold?" Now then do I contend, and though I give my reasons with great diffidence and respect to the opinions of those who have gone before me, yet am I firm in my convictions of the truth and permanency of the ground I have taken; namely that heat is the remote cause of fevers. Moisture and cold the exciting.

In the seventh volume of the American medical recorder for 1824 may be seen that excellent essay of Professor Cooke on epidemic fevers, and for the further illustration of my premises I will cite my reader to a table he has furnished us with, page 486, shewing the medium temperature of the weather in Philadelphia in the months of June, July and August, from the year 1793 when the yellow fever first commenced its ravages, to the year 1817 when it had subsided.

In the year 1793, Medium height of the Thermometer 82° 2' —Deaths 4000.

From this table it may be seen that the mortality was greater in that year when the Thermometer was highest and vice versa. The next year the medium temperature of the Thermometer was three degrees lower. "A few deaths about dirty places and the ponds of the suburbs."

In the year 1798, height of Thermometer 81° 8'.

Deaths, 3500.

Who can examine this table without being struck with the remarkable correspondence of mortality with the height of the thermometer? The difference between the medium temperature of the months of June, July, and August in the year 1793

and 1798, was not one degree; and yet we see a difference of 500 in the bill of mortality.

Again in the year 1804. Medium temperature 74° 3′. "Uncommonly healthy."

From the year of 1806 to 1817, I will also give the medium temperature, as recorded in this table.

Medium temperature 75° 7′.

"During a part of the time unexampled health particularly in the cool summers of 1814 and 15."

It appears that the summers of 1814 and 15, were remarkably cool and that during that time the health of the city was unprecedentedly good.

To what cause then are we to attribute this state of things? Must we say there was not enough of heat and moisture for carrying on the process of putrefaction? The medium height of Thermometer during the months of June, July, and August in 1814 and 15 was not probably much above seventy. One thing however we are certain of, that, in every case, the absence of excessive heat was attended with the happiest influence on the health of the city; and we have no proofs that putrefaction was not going on, though not in so high a degree.

In the year 1798, from the same authentic table we learn the summer was very dry (a state of things not favourable for carrying on the process of putrefaction) for though the heat of the summer was amply sufficient, yet the remaining equally important ingredient, moisture, was wanting. The deaths from yellow fever as before stated was notwithstanding 3500.

Again, in the year 1805 the summer was exceedingly dry.

Thermometer 79° 7′.

Deaths 300 or 400.

There again we find wanting, as in 1798, that due mixture of heat and moisture, but as the thermometer was more than two degrees lower, we see that the number of deaths was proportionally lower.

It appears that it was the decided opinion of Dr. Rush that this fever derived its origin, from the wharves and other filthy places in the city where vegetable and vegeto-animal matter lay in

a state of putrefaction. Yet many honestly differed from him in sentiment; indeed a large proportion of the city, whose interest might have led them to believe otherwise, were opposed to his opinions and ascribed its origin to other causes, and its prevalence and continuance to contagion.

When wise men and philosophers differ, who shall decide?

The tide of popular opinion has long since turned in favor of the sentiments of Dr. Rush. An obscure practitioner would also give his sentiments, and ascribe not only to autumnal fevers but likewise to yellow fever the same remote and exciting causes, heat, cold and moisture. I would not pretend to say, that there were not other exciting and occasional causes. These however should only be considered as occasional or accidental.

Cholera and dysentery, diseases which are justly attributable to the same exciting agents as autumnal fevers, will not, I presume, require the same amount of testimony to prove that they too have a remote cause, heat, (promoting and exciting the secretions from the skin) and exciting causes, a damp, cool and humid state of the atmosphere, driving in upon the bowels Sydenham's fever; or more consistent with our present pathological views, checking suddenly those secretions and forcing them in upon the equally relaxed and debilitated viscera. Hence in diseases dependent upon these causes, we so frequently see upon post mortem examinations, traces of ulceration, inflammation and mortification in the mucous membranes of the stomach and bowels, and an engorgement or congestion of the liver, spleen, &c. But, says a popular theory, the poisonous effluvia has been swallowed into the stomach and on that sensible and irritable part exerts its first deleterious influence—from hence communicated by sympathy or some other inexplicable manner to all the other parts of the system that afterwards become implicated.

Now it would appear to be much more rational, if marsh effluvia exists in a gaseous state, to say that the respiratory organs whose peculiar province it is to receive and separate the gases in the act of respiration, were the organs first affected,

mixture of heat and moisture necessary for carrying on the process of putrefaction: but does this prove that vegetable putrefaction is the universal cause of epidemic fevers?

Reasoning by the same rule of induction then, the epidemic would not only be more general, but the cases most violent and unmanageable. That cases are more general in districts of country most favourable for carrying on the process of vegetable putrefaction is most certainly true; but this I will presently make an effort to shew, may be owing to other equally appreciable and more tangible causes.

That cases are most violent in districts of country favourable for carrying on the process of vegetable putrefaction, cannot be proven: my limited experience teaches me otherwise, and I have but little doubt that universal experience will coincide with me in this opinion: the most unmanageable cases, that I have seen, have been in comparatively elevated situations, sporadic cases. Our mountains and hill country can respond to the truth of this assertion, and it has almost become proverbial among practitioners inhabiting low and marshy countries, that their cases of fever though numerous are generally manageable under judicious and well directed practice. Cannot equally as good reasons be produced to prove that moisture coming in contact with an exhausted, fatigued, relaxed and debilitated constitution, may be as general a cause of fevers as marsh effluvia? And when this cause is preceded by the relaxing, exhausting, and debilitating remoter causes excessive heat; are not the two combined, sufficient in themselves to produce fever? This in the humble opinion of the writer is the bugbear that has annoyed us for ages past, and that no age will ever be exempt from, and that no foresight or knowledge can eradicate.

Whether that peculiar gas or something extricated by the decomposition of vegetable substances has ever produced one case of fever is extremely doubtful. It appears to me much more rational and consistent with our present pathological views of the operation of deleterious agents on the human system to consider as the general cause of epidemic fevers, the compound influence of heat and moisture.

much for the medical world, admits in effect the questions in point—but further he observes, that dysentery is a form of the bilious remittent fevers of the summer and autumn.* The above quotations go far in establishing the fact, that dysentery is only another form of the same fever; but go much farther in establishing the position that it too derives its origin from the same remote and exciting causes. In cholera I would not venture to assert that there are not other exciting causes besides those above designated, but would place this disease upon the same scale with fevers as to the remote cause. Dr. Gregory distrusting all that preceding writers have said upon the subject, remarks, that the proximate cause of this disease is still unknown.

But Dr. Potter is more positive on this question, and says, "that its causes were cold drinks, crude and indigestible fruits eaten in great quantities, as melons, cucumbers, pears, apples, oranges, cold and moisture applied to the feet, suppressed perspiration, marsh effluvia, great external heat producing a secretion of bile, active medicines as purges and emetics."

On these remarks of Dr. Potter I shall make no further comment, than by placing heat as the remote cause, cold and moisture as the common exciting causes: some of the exciting causes he has mentioned, act only in subordination to the remote cause, and would not produce the disease with its characteristic marks, were it not for the predisposing and debilitating influence of heat. Hence we find the disease in the time of Sydenham, confined mostly to the month of August. Before leaving the subject of cholera, I cannot refrain from making a quotation from Cullen, in favour of the sentiments I have advanced. After giving some vague idea of the nature and causes of this disease he observes: "It has been remarked, that in warm climates and seasons, after extremely hot and dry weather, a fall of rain cooling the atmosphere, seems especially to bring on the disease, and it is very probable that an obstructed perspiration may have also a share in this.†" By turning back to the quotations I have made from Doctors Gregory and Cald-

* Gregory's Practice, vol. 1, p. 466.
† Caldwell's Cullen, Sec. 1460, vol. 2.

well on dysentery, the reader may see the remarkable correspondence between the causes by them assigned as producing dysentery, and those just cited from Cullen on cholera. But why search the pages of medical history on these questions? let every practitioner call to his aid his own experience in these diseases, and I am persuaded if he will throw off the shackles of prejudice, and the despot marsh effluvia, he cannot much longer doubt the truth of these assertions, but will be willing and constrained to believe, that cholera, dysentery, yellow fever, intermittent and remittent fevers, are all attributable to the same remote and exciting causes.

What I have thus with great diffidence committed to the scrutinizing eyes of an enlightened public, is the result of my deliberate convictions. I have now seen so many cases of fever, which by the most careful investigation, I could not trace to marsh effluvia, that I am forced irresistably into these conclusions.

ART. VIII.—*Remarkable case of Hæmorrhage.* By JOHN TERRILL LEWIS, M. D. *of Woodford County, Kentucky.*

ON the 4th of January 1830, I was called in haste to see Mrs. S. of Scott county, Ky. who was represented to be dying from the loss of a large amount of blood, thrown up in vomiting and passed from the bowels and bladder. I found her extremely cold, pulse scarcely perceptible, and 120 per minute, with great anxiety of countenance, jactitation, and a compressive pain in the præcordia and right hypochondrium. Early in the morning her urine was considerably coloured with dark blood, and two discharges of clotted blood took place from the bowels about the same time.

Two hours before my arrival she had vomited nearly half a gallon of black venous blood, part of which I saw, the balance

having passed through an opening in the floor. A pill composed of the super-acetate of lead and opium had been given by a medical gentleman who was called in before me.

The case was confided entirely to my care, and from a history of her situation previous to the attack, in connexion with the symptoms at the time, I felt well assured that the hæmorrhage proceeded from a highly congested state of the vena cava and its branches, by which the capillary vessels in the bladder, stomach and bowels were *forced.* From this view of the case, I undertook to attain two indications, viz. first to excite increased arterial action, and second, to operate on the liver. By the free use of hot brandy toddy, frictions, and warm applications to the extremities, her pulse became sensibly better in the course of an hour. Believing that the second indication could be attained only by calomel, and from the urgency of the case, the necessity for administering doses that would act both promptly and efficiently being palpable, I prescribed it in a dose of ʒj. with instructions to continue the toddy so as to keep her comfortably warm.

Jan. 5th. The calomel had produced two discharges of matter resembling a mass of wet gun-powder, which on examination proved to be blood. Pulse still quick and below the healthy standard. A considerable quantity of blood had been thrown up in vomiting. Her extremities and lower part of the abdomen were marked with dark livid spots of various sizes; and she complained of distressing nausea and great prostration of strength. I gave calomel ʒj. and as no sensible effect had been felt in the bowels from the previous medicine, I ordered that two pills, composed of equal parts of calomel, aloes and rhubarb be given every other hour, and the toddy continued as before.

Jan. 6th. Pulse much as it was yesterday; nausea still very distressing, with occasional attacks of singultus. Had four very large discharges of black matter, slightly tinged with green. A considerable amount of black matter, having the appearance of putrid blood, was passing from the vagina; and the milk drawn from her breast darkened with blood. Prescribed the same means as on yesterday, with the addition of three

pills to the two before given. I was sent for at 11 o'clock, P. M. in consequence of her having vomited nearly a quart of matter similar in character to that which passed by the bowels; but in addition to the course already directed I only applied a blistering plaister over the stomach, for the purpose of allaying the irritation of that organ.

Jan. 7th. No perceptible change in her pulse. Plaister had drawn well, and the sickness and singultus were less distressing. Milk not mixed with blood, but the vaginal discharge considerably increased. Alvine discharge more healthy. Prescribed the same course as on yesterday.

Jan. 8th. Bowels well opened and the discharges much improved in colour. Pulse nearly at the healthy standard. Slight ptyalism had been induced, and the discharge from the vagina evidently menstrual. The spots discovered at my second visit, had assumed a greenish yellow cast. The calomel and toddy discontinued, but the pills given as before.

Jan. 9th. Found her much better, alvine and menstrual discharges entirely healthy. Pulse good, and she complaining only of weakness. Ordered that the pills be taken so as to produce two operations daily.

Jan. 10th. Found my patient eating and in fine spirits. Instructed her to continue the pills as before directed, until her health should be quite restored. Her menstrual discharge ceased in a few days, and she is now, as I am informed by Mr. S. in the enjoyment of better health than she has had for a year past.

Mrs. S. is of rather a delicate habit of body æt. 24, fair complexion, light hair, and blue eyes. Her only child is an infant 12 months old, at the birth of which she was very much injured by the officious interference of the midwife; and has since been afflicted with hæmorrhoids. She had gotten her feet wet on Christmas day, and from that time until she was attacked, continued unusually pale, with a distressing pain and throbbing sensation in the right side of both her chest and abdomen. Those symptoms became gradually more aggravated until the hæmorrhage took place.

ART. IX.—*Cases illustrating the utilities of the* BANDAGE, *as a Surgical agent.*

[Professor Dudley has placed in our hands a number of letters received from different quarters attesting, in the strongest terms, the correctness of his principles and the success of his practice with this important agent.—For the present we give the following. EDS.]

In secondary Hæmorrhage &c. By FRANKLIN WILLIAMS, M. D. of Cambridge, S. Carolina.—On the 22d of June last my brother and co-partner Dr. John W. Williams, assisted by Dr. Calhoun and myself, amputated Mr. Steel's leg just below the knee, (performing the double flap operation,) on account of the ravages committed on the lower and outer part of the leg and fibula by an ulcer of some standing, and from the rapid manner in which his constitution was yielding. About 5 A. M. on the 29th, hæmorrhage occurred but was very soon arrested by the tourniquet. Not being apprized of the occurrence until 10 o'clock, it was 12 when my brother and I reached him. I proposed the bandage as used by yourself, rather than the knife and ligature, which, if it failed, would not diminish the chance of success by the latter; to which he readily consented. On removing the dressing we found the flaps had adhered their whole length, except where the ligatures were brought out, and that one of the ligatures brought out in the centre was missing. From the size of the stream of blood it was thought to be the anterior tibial artery which was bleeding. After placing compresses along the entire length of the femoral artery, I commenced below the knee and applied the roller, as tight as I could conveniently do, up to the perinæum, then round the pelvis to secure it. As the tourniquet had been applied about 7 hours, the limb below its application was very much enlarged. In two hours I was summoned to the patient, as hæmorrhage had again taken place. I found the bandage over all the

enlarged part of the limb so loose that I could pass one of my fingers with ease under it. I took another roller, and commencing at the stump, applied it firmly over all the enlarged part of the limb, which was now as small as the upper half; which again arrested the hæmorrhage.

At 10 A. M. on the 30th, no hæmorrhage; bandage loose. Having the artery compressed where it passes over the os pubis I removed the rollers and applied one firmly after the manner of the first on the 29th. I then dressed the stump and bound it up.

At 8. A. M. on the 2d. July, no hæmorrhage; bandage on the thigh pretty tight; not interrupted. On removing the dressing from the stump I found the ligatures had all come away, and the orifices closed except the one in the centre; but there was between half an ounce and an ounce of pus contained in a pocket beneath the remaining orifice, towards the depending part of the limb. After dressing it on the plan of the 30th, I turned a short roller over the end of the stump so as to obliterate the cavity and avoid pressure on the tibia, lest I should produce ulceration of the integuments covering it. On the 5th, the bandage on the thigh loose, but I did not interrupt it; the secretion of pus considerable; the cavity which contained pus before, entirely obliterated. I dressed the stump as on the 2d.

On the 8th, no cavity, dressed as before, patient allowed some exercise.

On the 12th, roller removed from the thigh; stump nearly well; patient's health very much improved; allowed to go on crutches. We see in this case that two applications of the bandage had the desired effect of arresting the hæmorrhage, and one in obliterating the cavity containing the pus.

In contused and lacerated fracture of the arm. By James M. Cunningham, M. D. *of Jefferson County, Georgia.*—Whilst Mr. M. was engaged in hauling wood, his team became frightened and ran away: being unable to disengage his hand from the line, he was dragged a considerable distance by it. The wheel of the wagon finally ran over his arm, lacerating the inte-

guments and muscles from the middle of the os humeri to some distance below the elbow joint.

When the injury was examined, which was six or seven hours afterwards, the limb was so much swollen that it was impossible to ascertain the extent of the injury. The patient supposed he had lost nearly a gallon of blood from the lacerated parts of the limb, which were still bleeding profusely. In addition to the swelling, the limb presented a dark black appearance throughout its whole extent, caused by the effusion of blood through the cellular texture below the integuments. Finding that a successful examination of the bone was impracticable, a bandage was immediately applied, extending from the fingers up to the shoulder. About thirty hours afterwards the dressing, which had become slack, was re-applied. On its third application it was ascertained that the wheel of the vehicle in passing over the arm had fractured the external condyle of the humerus. The arm now presented a more natural appearance; the effusion was gone; the swelling greatly diminished; and the lacerations manifested a disposition to healthy cicatrization. The bandage was the only dressing applied. The patient soon recovered the use of his arm under the above treatment; although under any other, the loss of his limb would have been inevitable, and the danger of losing his life imminent.

In Fracture and Fistulous Ulcers. By WILLIAM T. YOUNG, M. D. *of Triana, Alabama.*—I have had this fall a case of fractured fibula, in a small boy, and treated the case successfully by the bandage, applied as Professor Dudley directs. The bone was broken about its middle transversely—the cure was effected in a less period than I ever knew one by the old method of dressing. I have also used the bandage in other species of fractures of the bones of the extremities, with the most speedy and successful termination. I have now a case of fistulous ulcer of the thigh, of eight years standing, which has baffled the skill of several able physicians, that I am treating by the use of the bandage, and from the favourable change wrought in the ulcer

already, by three applications of it, I feel every confidence in a successful issue of the case.

November 26th, 1829.

ART. X.—*Case of Remittent Fever attended with large discharges of grumous blood from the bowels, successfully treated with large doses of Calomel and Quinine.* By Y. D. BOLLING M. D. of Memphis, Tennessee.

E—F—ætat. 21, was on the 15th September attacked with the usual symptoms of remittent fever. His case not exciting any alarm at the commencement, was entrusted to the management of an old woman in the family: the medicines administered by her, were a dose of salts, which operated well on the bowels, as she informed me; a composition of vinegar and glass of antimony, called by her crocus, given as an emetic and diaphoretic, which likewise produced the desired effect. Still the patient grew worse, and on the night of the 20th September I was sent for in haste to see him.

I found him covered with a profuse clammy sweat, the extremities cold, pulse frequent and small, some hiccough, eyes thrown back when he would attempt to close them, and discharging from the bowels large grumous passages. Convinced of the dangerous situation of my patient, I intimated to his friends the necessity there was for the recurrence of bilious evacuations, or he would soon sink irrecoverably. The indications, as I conceived, were to produce a decided impression on the liver with calomel, and to excite the dormant energies of the system with quinine. Having met with such cases before, I was aware of the danger there would be to the patient in trusting to any medicines in ordinary doses. He therefore took sixty grains of calomel, and a short time after five grs. of quinine. These doses were repeated twice afterwards, in the course of the night. There was no bloody evacuations after the first dose; the warmth of the skin became equable; the perspiration receded, and in

the morning early, he had a large, green, bilious passage, with not a particle of blood intermixed. This advantage obtained over the disease, was maintained by calomel, rhubarb, aloes, and quinine, alone, given in ordinary doses. The disease was now divested of all malignity, and the patient soon recovered.

Observations.—Whether the blood in such cases proceeds from the distended veins of the liver or intestines, the writer is unprepared to say, as he has made no post mortem examinations; but is inclined to believe it to be a morbid secretion of the liver, as there is, sometimes, a discharge of blood and bile alternately, and never an admixture of them. The patient complains of no pain in any of the abdominal viscera. The evacuations are at regular intervals as if from the operation of a purgative, discharged freely and at once. Such cases are well calculated to alarm the practitioner when he first meets with them, and render him undecided, when delay is death to the patient. The patient and his friends are alarmed at such an unexpected occurrence, and now is the time for cool deliberation, decided and energetic practice in the physician. I have known one physician under these circumstances, to recommend bark, and another calomel in two grain doses.

ART. XI.—*Case of Laryngotomy.* By Dr. JAMES F. E. HARDY, of Ashville, North Carolina.

EARLY in the month of September last, I was visited by William Welch, of Haywood county N. C. who had come a distance of sixty miles with his son John, aged two years and ten months; who had ten days before, while eating some water-melon, inhaled one of the seed into his trachea: he became instantly suffocated, and for some hours his recovery was doubtful: he, however, revived; but during the whole of the ten days, he had frequent and alarming attacks. The day I first saw him he had a paroxysm, which I thought would have terminated

fatally. I then informed the father of the only operation by which I thought his child could be saved; and he being an intelligent man was soon made to see the necessity of it, and readily consented to its being performed.

The day following, at 10 oclock, I commenced the operation in the presence of several medical gentlemen from South Carolina and Georgia, who were at that time spending the summer months in our village. My instruments consisted of a small scalpel, a bistoury, canula, and two bougies. I first made an incision through the integuments, two inches in length, the age and size of the patient rendering it necessary to make a large opening. This being accomplished, I opened the cricothyroid membrane with the bistoury, which was simple and easy. I found, however, my opening was too small to give me any chance to search for the foreign body. By the advice of the gentlemen present I continued the incision through the thyroid and cricoid cartilages, to the full extent of the external opening. Immediately on completing this part of the operation, most alarming and dangerous hæmorrhage took place, as I supposed from the superior thyroid artery: the blood immediately rushed into the trachea, producing complete suffocation, and my little patient was to all appearance dead. I immediately introduced the canula into the opening in the larynx, and gave it to one of my medical assistants who made powerful efforts to inflate the lungs, while I took up the divided vessel. The bleeding being arrested, I assisted him in trying to produce artificial respiration, which we ultimately accomplished in twenty-five or thirty minutes, all of which time I thought the child was closed in death forever. He continued to breathe through the canula for about one hour: we then permitted him to have one hour's quiet while he slept. Yet his breathing was laborious and difficult. When he awoke I made a minute examination, and to my great mortification could find nothing of the obstruction; when, upon the suggestion of one of my medical friends, I passed a bougie through the artificial opening into the mouth, which to my equally great satisfaction, carried the seed with it. The after treatment was simple. I declined bringing the wound together by

suture, the trachea had become so extremely irritable that I was fearful of inflammation. I drew the lips slightly in apposition and retained them by adhesive strips, over which I placed pledgets of lint: this dressing had to be renewed daily, as the mucous discharges were so profuse as to soften the adhesive plaister. He had a constant wheezing and cough for two weeks, bnt no fever, and returned home in three weeks, the wound healed with the cicatrix small and well shaped. His voice is perfect and distinct, and his health good.

Remarks.—Since the operation I have had the pleasure of seeing a case recorded in the American Journal of the Medical Sciences, by Abner Hopton M. D. in which he particularly guards all operators, against the alarming and often fatal hæmorrhages that take place in dividing the thyroid artery. I am well convinced, from experience, that his observations are good, particularly to young surgeons, for had I seen his publication previously I should have been more cautions.

ART. XII.—*Case of Dyspepsia.* By JAMES LYNE, M. D. of Mississippi.

[COMMUNICATED IN A LETTER TO PROFESSOR COOKE.]

Dear Sir—Permit me to relate to you a remarkable case of dyspepsia that has presented itself to me since I had the pleasure of hearing your lectures of winter before last.

Shortly after the conclusion of that course of lectures I accidentally fell in with the father of the little girl, who is the subject of the disease. He gave me the following history of the case. In June 1824, his little daughter, then about nine years old, had an attack of fever. Her physician in his treatment gave her an emetic, which produced inordinate vomiting—it was not checked for several days, and prostrated her very much. After she improved a little, her appetite became exceedingly

voracious, and her friends indulged her in whatever she wanted. This, with some other symptoms, induced her physician to think her the subject of tape-worm. Soon after this she commenced spitting up her meals shortly after swallowing: this circumstance changed the name of the disease and the practice. Another physician was called in consultation: the disease was pronounced dyspepsia, and was treated by the usual means. They completely exhausted their catalogue of anti-dyspeptic remedies, without in the least benefitting the poor girl; when a third one was called in from an adjoining county. He had her in charge twelve months, and left her as he found her. The father at this time began to despair of having his daughter cured by Doctors, and determined to trust the case pretty much to nature. He was however under the necessity of giving her something to keep her bowels open, and for this purpose he generally used salts or oil. I asked him what effect they had upon her: he said they produced dark coloured stools, which he attributed to her taking so much cold. He said she complained of great heat in her breast, in the palms of her hands, and soles of her feet, and that in the coldest weather she would lay before an open door, or under a hoisted window.

He had been giving her salts and oil for twelve or fifteen months when I first saw him. Her friends were in the habit of indulging her in whatever she wanted, in the way of diet. She would eat a hearty meal and spit it up in five minutes afterwards. At the age of thirteen she weighed thirty-three pounds.

I related to the father some of the cases that you had put us in the possession of; and advised him, as he was forced to give her something to keep her bowels open, to try pills of rhubarb, aloes, and calomel. He consented, and I made him several dozen and gave him directions to make her more when those gave out.

Soon after this I went to the south and heard no more of the case till I returned to Kentucky last spring. The first time that the father saw me after my return he informed me, that the pills had helped his daughter very much, that he had lost my directions and was forced to return to his salts and oil again, and

said she was no better than when I left her. He desired me to make her some more pills. I gave him a large box of them, and told him when they gave out to get the same materials and make more, or to make some extract of white walnut, and mix it with a small portion of calomel. Enough of these pills were given every night to operate on the bowels; two or three were generally sufficient. The last time that I saw her was in the latter part of October, when she was much better,—had gained considerable flesh, and was able to walk around the room by means of a *go-cart*, a thing that she has not done for five years before.

During the Christmas holy-days I was in the neighbourhood of her father's, and hearing of the rapid improvement in his daughter's health I determined to go and get a minute history of the case; but before I arrived at the house I met the father and mother going to church, and as I was in haste to return to Lexington, could not wait their return.

The father informed me that his daughter could walk where she chose about the house, and that she had ridden four miles alone—he said he had never seen any one improve more rapidly in their recovery from a fever, than she had done in the last month, or since she had got so that she could walk about.

But few cases show more clearly the importatnce of purging in dyspepsia than this one. I regret that I have it not in my power to give you a more minute account of the case.

Yours very respectfully

JAMES LYNE.

ART. XIII. *A Case of Labour in which the Placenta was delivered some time before the Fœtus.* By DR. CALEB W. CLOUD, of Lexington, Kentucky.

ON the 3d of February, 1830, at 11 o'clock, P. M. I was called to visit a woman, who was said to be in labour. I found her in considerable pain, with great uterine hæmorrhage: she said her labour was premature, and that she was in the eighth

month of her pregnancy. On examination per vaginam, I found the os uteri considerably dilated; one knee of the fœtus, and a part of the placenta could also be distinguished. The pain subsiding, I gave her syrup of rhubarb and laudanum, and left her for the night.

At 8 o'clock A. M. of the ensuing day, I saw her again, when the pain was slight, though the hæmorrhage continued. At 12 o'clock M. the uterine pain was increased and the bleeding had become quite alarming. On examining again I found the membranes protruded, which were soon afterwards ruptured, when a large portion of the placenta together with the umbilical cord, presented. Pulsation was felt in the cord, but no portion of the child could be reached.

In this situation, I gave her fifteen grains of ergot, and after an interval of 20 or 30 minutes, the same quantity was repeated. In half an hour from the exhibition of the last dose, the placenta was expelled, and the hæmorrhage immediately ceased. Still the child could not be felt, per vaginam, and I had some apprehension at the time that it might have escaped into the cavity of the abdomen, by a rupture of the uterus. The woman, however, being a good deal fatigued, was permitted to rest for two hours or more; when, there being no return of pain, or descent of the fœtus, I gave her fiteen or twenty grains of ergot, and repeated the same quantity in twenty minutes. Some pain now came on; but, being slight and ineffectual, I introduced the hand and dislodged the child from the os pubis, on which it rested, inclining to the right side: after this I was enabled to reach it with the forceps, (the breech presenting,) and in this way it was delivered.

After delivery the woman complained of great debility, with sick stomach and vomiting—these, however, were soon relieved and she recovered speedily. The child was well formed and of the usual size. The mother is about 37 years of age: this was her eight pregnancy, all the preceding labours being natural; and she knows not of any cause to have produced premature or preternatural labour in the present case.

ART. XIV.—*Case of Bilious Fever.* By J. F. FLETCHER, M. D. of Salvesa, Mercer county, Kentucky.

MISS P. Armstrong aged 18, residing near this village, had for some months laboured under delicate health: at length she was attacked with the ordinary autumnal fever of the country, which obstinately resisting a variety of domestic medicines, on the 13th of July my assistance was requested. I found her under great depression of spirits; skin pale and inclined to coldness; some head-ache; much uneasiness about the precordia; great thirst, with loss of appetite; tongue loaded; pulse feeble, compressed, and alternating; with considerable torpor in the bowels, and paleness in the alvine discharges. With the view of unlocking the portal circle, and restoring the biliary secretions, I gave her 40 grains of calomel, aloes, and rhubarb, directing her attendants to repeat the dose if no operation was obtained by bed time.

July 14th. No operation; premised venesection; ordered an injection; a scanty thin stool obtained; gave two scruples of calomel, and at four o'clock directed that she should take two scruples more, aided by oil, and injections.

15th. No important change in the symptoms; ordered the warm bath. Eight tablespoonsful of castor oil had been taken, and half a dozen injections administered, without effect. She now took 15 grs. of scammony and 15 grs. of jalap every six hours.

16th. A thin discharge this morning—pulse more feeble, system highly irritable, appearances discouraging; gave her during the day and early part of the night, large doses of scammony and croton oil: at 11 P. M. a copious thin discharge obtained, succeeded by vomiting and alarming debility. Wine whey, camphorated julep, and sinapisms to the stomach and extremities were resorted to.

17th. Continued the wine and julep. The extreme irritability of the stomach this morning, which heretofore had surprisingly retained every thing administered, and the evidently sinking state of my patient, strongly admonished me of the necessity of a different plan of treatment. I now stated to her friends, her imminent danger, explained in detail my views of her critical situation, and assured them of my firm belief, that nothing but calomel freely administered presented the slightest prospect of saving her. I used no concealment, but frankly suggested all the consequences that might accompany this practice. After some hesitation I was fully authorized to take my course without restriction. Consistent discharges being the great object, at 4 P. M. I gave her two drachms of calomel, which appeared to have a happy effect in soothing the irritability of the stomach.

18th. No operation this morning: pulse however slightly improved, and system more calm. Gave two drachms of calomel; at twelve o'clock two drachms more, and pills of scammony in the evening.

19th. Two small and consistent but highly fœtid operations procured by stimulating injections: gave her three drachms of calomel, followed at 8 P. M. by an ordinary enema, which brought with it a large discharge of the darkest and most offensive matter; administered cordials freely. Patient's gums slightly affected by the mercury.

20th. Another free discharge, of the same character as the last obtained by injection; pulse better and my patient's strength and spirits materially improved; ptyalism inconsiderable.

20th. Procured within the last twenty-four hours three consistent stools by injections: symptoms favourable. Gave a drachm of calomel.

21st. No operation since yesterday morning, patient more dull. Gums nearly well, gave two drachms of calomel, to be assisted as usual by enemata.

22nd. Several consistent discharges have been obtained *without injections.* Patients appetite reviving, with an evident increase of strength and cherfulness. Gums sore but not tron-

blesome: directed her bowels to be kept open with pills of aloes and rhubarb assisted by oil if necessary.

25th. Patient recovering fast, appetite good. Gums well.

30th. Able to take exercise in the house.

August 10th. Patient rides out visiting her friends and is convalescent.

Nov. 1st. Has entirely recovered her health and spirits.

I make no comment upon this case. I have given a plain statement of facts. And in relation to the doses of calomel administered refer to Mr. A. Armstrong, brother of my patient and one of the most respectable citizens of Mercer county, who under my directions invariably weighed the calomel and gave it himself.

MISCELLANEOUS INTELLIGENCE.

Columbo of the Materia Medica.—This is the root of a plant which appears to have been long known in some parts of the East Indies, but whose native country and history have been, till very lately, involved in obscurity. Some have asserted, (see Rees's Cyclopædia, art. *Columbo,*) that the root was introduced from Columbo in Ceylon, to the continent of India, and thence derived its name. If such were the case, Ceylon was not considered, by the learned in India, to be the native country of this plant; for the Asiatic Society of Bengal, in the sixth vol. of their Transactions, published at the commencement of the present century, gave a "list of such Oriental subjects as required further illustration;" and amongst them particularly invited communications respecting the *Columbo plant*, with a view to ascertain from what country the root so called was originally procured, and to obtain a botanical description and figure.

Commerson, indeed, while residing in the Isle of France, gathered there, in the garden of M. Poivre,* about the year 1770, some flowering specimens of a plant which came to France with the rest of that celebrated botanist's collection, and were marked in his Herbarium "*Calumbo in Indiis vocatur*;" and these were first described by Lamarck, under the name of *Menispermum palmatum.* That author further observes: "Il paroit qu'elle est originaire de l'Inde," and "C'est peut-etre de ce *Menisperme* qu'on obtient cette racine qu'on nous rapporte de l'Inde sous le nom de *racine de Calumbo* ou Columbo, en morceaux jaunatres, d'une saveur amere." From no other source do we derive any indication of the plant being known in a living state about that period; and *that* in the Isle of France probably soon perished. About eight or ten years after the invitation was given on the part of the Asiatic Society, for information respecting the Columbo plant, (namely, in the tenth volume of the Asiatic Researches, 8vo. edition published in London, 1811,) we find a figure and description of the male plant, and some important contributions to its history, by Dr. Andrew Berry, Member of the Medical Board of Fort St. George. From that gentleman we learn that this plant is never *cultivated* in its

* See Lamarck's Cycl. v. 4. p. 99.

native country, but that it grows naturally and in abundance in the thick forests that are said to cover the shores about Oibo and Mozambique, on the east coast of Africa, and inland for about fifteen or twenty miles. The roots are dug up in the month of March, the dry season, or when the natives are not employed in agriculture. The main stock of the root is not removed, but offsets from the base are taken, and those of sufficient size: yet not so old as to be full of fibres, which render it unfit for commerce.

Soon after it is dug up, the root is cut into slices, strung on cords, and hung up to dry in the shade. It is deemed fit for commerce, when, on exposure to the sun, it breaks short; and of a bad quality when it is soft or black. It is held in high esteem among the natives, even those who reside at a great distance from Mozambique, for the cure of dysentery, which is frequent among them; for healing ulcers, and as a remedy for almost every disorder.

Dr. Berry obtained this information from Monsieur Fortin, who, when at Mozambique, and engaged in purchasing the dried root, as an article of trade, procured an entire living offset of a larger size than usual. This he brought with him to Madras, in September, 1805; and presented it to Dr. James Anderson, the Physician General, who justly esteemed it a valuable acquisition to India. This being planted, produced a male plant, from which Dr. Berry's figure and description were made. That gentleman was ignorant of the circumstance of Lamarck having published it from dried specimens, as the *Menispermum palmatum*, nor has he given it a scientific name; but he rightly judged it to belong to "the Sarmentaceæ of Linnæus, the Menispermum of Jussieu." Among the Africans its name is *Kalumb*. It is spelt *Kalumbo* by the Portuguese, in whose language the *o* is mute; and, from this, Dr. Berry tells us, the name originated by which this valuable root is known to Europe. It is a staple export of the Portuguese from Mozambique; and, when we consider the quantity exported, it is certainly an extraordinary circumstance that the native country of the plant was not more generally known.

Still the female flower was a desideratum; and De Candolle, the last botanical author perhaps who has described the plant (apparently from Commerson's specimens in the Parisian Museum,) was obliged to declare that he was ignorant of the structure of the female flowers and fruit.

For the means of making them known to the botanists of Europe,

I am mainly indebted to the indefatigable exertions of my often-mentioned friend Charles Telfair, Esq. of the Mauritius. From that gentleman, through the hands of Robert Barclay, Esq., I have received no less than three atlas folio sheets of drawings, representing portions of this plant of the natural size, with admirable dissections, executed by Professor Bojer, aided in the colouring by the faithful pencil of Mrs. Charles Telfair. Whatever is wanting, either of execution or accuracy in the accompanying figures, which were taken from these splendid delineations, must be attributed to my being obliged to reduce them to the humble standard of the Botanical Magazine. These drawings too were accompanied by a full description of the plant, from the pen of Professor Bojer, (and which I have here merely rendered from the Latin,) and by an account of the recent introduction of the plant to our colonies, from a M. S. memoir, by Mr. Telfair himself. Shortly previous to their arrival in England, living roots were sent by that gentleman to Mr. Barclay's Botanical Garden and to that of Glasgow.

"The late Sir. Walter Farquhar, Physician to the King," says Mr. Telfair,* "was very anxious to introduce into England the *Colombo root* in a living state; and for that purpose, he desired his son, Sir Robert Farquhar, Governor of Mauritius, Bourbon, and their Dependencies, to procure the plant from its native soil in Africa, aud forward it to London. Sir Robert lost no time, after assuming his government at the conquest of the French Islands, in applying to the Governor of Mozambique for growing plants; and was repeatedly assured that these should be sent to him at the proper season. These promises, however, were never fulfilled; although renewed by the several succeeding officials of the Portuguese possessions on the east coast of Africa, ever since the year 1811 Dr. Wallich also took much pains for effecting the same object, and sent to Governor Farquhar the drawing made at Calcutta of a male plant of the *Colombo root* which had been brought to the Botanic Garden there by Mr. Berry. Copies of this drawing were distributed to the different ships of war and captains of merchant vessels, trading to the eastern coast of Africa, that they might be enabled to distinguish the plant and bring it to the Mauritius; since there had been an evident unwillingness on the part of the Portuguese authorities to permit this precious vegetable to be taken away,

* This account appears to have been read at a literary and scientific Society in the Mauritius, of which Mr. Telfair is the Vice-president.

in any other state than what it bears in commerce, when deprived of vegetative power by passing through the oven.

"All the attempts resulting from these means proved fruitless; until Captain William Fitzwilliam Owen, commanding the surveying squadron of his majesty's navy, on the East African coast, undertook the task. The extensive influence he had acquired by his intercourse with the native chieftains and tribes, enabled him to procure living plants; while his botanical knowledge secured him against the mistakes committed by others, who had been misled by the local settlers in their search, and imposed on by the substitution of other species instead of the true *Colombo root.* Captain Owen, in the year 1825, brought away in his Majesty's ship Leven, from Oibo, a great number of cases, filled with growing roots of the male and female plants, laid down in the sandy loam, which appears to be their favourite soil. No time was lost by him in forwarding a great portion of these to Mr. Telfair, at Mauritius, planting some also at Mahe, in the Seychelles Archipelago, and sending to Bombay several cases, in order to multiply, by dispersion, the chances of success in naturalizing them in different climates.

"The roots that were brought to Mauritius, were partly transmitted to England, New Holland, and America; but the greater number were distributed among the various districts of Mauritius and Bourbon. Many of these plants blossomed at Mauritius in the course of a year, but the flowers proved all male. The roots, however, had, during that period, multiplied to twenty or thirty times the original quantity, and thus an opportunity was given for distributing them still more extensively. The female plants flowered at Seychelles, and Mr. G. Harrison the Government Agent there, transmitted some of their roots to Mr. Telfair, in whose garden of Bois Cheri, in the Mauritius, they have flowered, and being fecundated by Professor Bojer, who touched them with the pollen of the male blossom, they bore seeds. From these individuals the drawings by Professor Bojer have been taken, which give a delineation and dissection of every part.

"Some of the original roots imported by Captain Owen, and immediately planted, have however remained in the earth without making any shoot, though they possess vegetable life, entire and undecayed. When taken up and examined, they appear to be undergoing a process of cicatrization at the surface, where they have been broken off from the parent root; and it is not until this wound is completely healed and firm, that the powers of vegetation are directed to the production

of a stalk and leaves and flowers. This process of cicatrization is extremely slow, thus offering an obstacle to the extensive propagation of this species by offsets from the root. But Professor Bojer is at present engaged in some experiments for increasing its productiveness, by laying down and planting the stem in cuttings, and hitherto with an appearance of success. The wound thus caused in the plant is much smaller, and seems to heal more readily. If this plan answers well, the culture of *Colombo root* may be so extended as to render it an object of industry and resource to the planters of the Mauritius.

"Mr. Newman, the Superintendent of the King's Botanical Garden of Mauritius, has prepared a tincture from a part of the roots in his possession, according to the formula of the London College of Physicians; but this tincture is much stronger than what comes from Apothecaries' Hall and its taste has a more grateful and aromatic flavour."

Columbo root has been long known and esteemed, both in the East Indies and in Europe; where it is found to be of singular efficacy in strengthening the fibres of the stomach and bowels, not only in chronic cases, but in the cholera morbus, dysentery, and diseases of the alimentary canal. Its analysis, according to M. Planche, has afforded a third of its weight of woody tissue, a third of amylaceous matter, and the remaining third is principally composed of animal substance and a yellow and bitter matter, insoluble in metallic salts. "The presence of so large a quantity of amylaceous substance accounts" for the great reputation of the root in dysentery, diarrhœas, and excessive vomiting. By a *decoction* of Columbo, the fecula are extracted, as well as the bitter principle, whose action is thus considerably modified: whilst an infusion is simply bitter and tonic.

In the Dictionnaire des Drogues by Chevallier and Richard (Paris, 1817,) mention is made of a *bastard* or *false Columbo* (*faux Colombo*), brought from the States of Barbary, but which may be detected at once on analysis, by the entire absence of fecula. It is not yet known of what plant this is the produce, but it is often substituted for the true Columbo, which it has thus sometimes unjustly brought into disrepute. *Dr. Hooker in Curtis's Botanical Magazine* (*London.*) *March* 1830.

2. *Curious Case of Ascending Paralysis; with the appearances on Dissection.* (*Clinique.*)

Case. Charles L. 35 years of age, robust and in the military profession for 14 years, during which he had served in the Russian and Span-

ish campaigns, and consequently been exposed to great fatigues and vicissitudes of climate. In June 1826, he first perceived that his legs readily bent under him, and that he could not easily raise himself up from the sitting posture. In other respects he had no complaint. In about a fortnight after this he began to feel a numbness in his feet, which gradually ascended towards the knee. But while the surface thus lost its sensibility, the muscles beneath became the seat of acute pain, which was much exasperated by pressure. He had been a month confined to bed in this state, with nearly loss of all power in the lower extremities, when he perceived a numbness invade his hands. The progress was exactly similar to that in the inferior members; and he was seen by the narrator on the 22d of September of the same year. He was now completely paralytic, excepting the tongue, the face and the neck. These last became gradually affected. He had never complained of pain in his head, nor of any part of the spine; nor did the most rigid examination detect any physical lesion in this last region. His general health, was good—his intellects perfect. He attributed his disease to rheumatism, contracted during his bivouacs in Spain. He made water voluntarily, and had a daily evacuation from the bowels. He slept and ate well. The skin was nearly of natural temperature, but quite insensible to pinching or pricking.—Any pressure of the muscles, on the other hand gave him great pain, and caused him to cry out.

Frictions of lytta and alcohol were assiduously employed along the spine—ammoniated liniments were applied to the limbs, and cinchona, with wine, was liberally exhibited internally. In the course of a fortnight the sensibility of the skin began to return and that of the muscles to diminish. The power of the muscles also gradually returned, but inversely to the way in which it had been lost—namely, from above downwards. He was never able, however, to raise himself up on his feet. This amelioration continued but for a very short time, and he was soon as bad as ever. Blisters along the spine were added to the former measures: on the third of November he became suddenly incommoded in his breathing—his pulse quickened—his countenance became anxious—he had cough—the intercostal muscles seemed scarcely to move. In this state he lingered till the 7th of the same month, when he expired without any struggle.

Dissection. The spinal canal was opened througout its whole extent. There was very little blood in the venous sinuses. The dura

mater in its natural state. The pia mater was sprinkled with calcareous depositions in the lumbar region, and was finely injected. The roots of the lumbar and sacral nerves, as also the great sciatic were injected with black blood. The other nerves were very minutely examined; but nothing particular was observed. The spinal marrow was rather firmer than natural, and the same might be said of the medulla oblongata and brain. The lungs were filled with tuberculous matters, and there were some small abscesses. The heart was empty and flaccid. The whole of the abdominal viscera were sound. The muscles presented no appearance different from those of a person in health, except being more pale and flacid.

The foregoing case will show with what a thick veil the functions and diseases of the nervous system are veiled. What was the nature of the malady? Was it inflammatory—or was it the reverse?—*Medico-Chirurgical Review. Jan.* 1830.

3. *Cataracts alternating with Diabetes.* (*Winchester county Hospital Reports, in Prov. Med. Gaz. No.* 11.)

The following is a very singular case, and one that would be met with incredulity, were it not guaranteed as it is, by the name of the surgeon and of the hospital.

Eliza Broomfield, æt. 15; remarkably tall and thin, was admitted with cataract in either eye. The pupils were dilated to their utmost, and the crystalline lens was evidently so augmented in bulk as to protrude through the pupillary opening; its colour was uniformly milky; the cornea unusually glassy; the vision so completely lost that the patient could only distinguish most imperfectly light from darkness. Independent of the cataracts, there were dyspnœa, cough, and loss of appetite.

The cataracts had appeared simultaneously fourteen months previous to her admission, and had been completely formed in the surprisingly short period of twenty days. The menses had appeared at the age of eleven, and had flowed with regularity till her thirteenth year, when they disppeared. From having been healthy and robust, she now became very debilitated, increased most astonishingly in stature, and subsequently was attacked with profuse nocturnal perspiration. When twelve months had elapsed an evident amendment took place; but it proved to be temporary only, and very shortly afterwards the girl began to complain of uneasiness about the head, with vertigo, confusion, and obscurity of vision.

℞. Infus. gent. c. ℥j. Spt. æth. nit. ʒss. Tr. camph. c. ʒss. ter die sumend. Pil. hyd. gr. ij. omni nocte sumend.

These medicines were continued for upwards of a fortnight with considerable benefit, and on the 20th of February we find that, though the cough was nearly gone, the dyspnœa and appetite were worse. The dilatation of the pupils, bulging, and milky whiteness of each lens were diminished, and the patient could readily detect any substance interposed between her and the light from the window. The medicines, were repeated with the addition of an aperient. On the 24th she could discern the flame of a candle at some little distance, but it was now discovered that the urine was too abundant.

"Feb. 28th.—The quantity of urine passed for the last four days has averaged sixteen pints daily. Its colour of a greenish yellow, and its taste strongly mellitic. Pulse 65, and feeble; appetite natural; dyspnœa not diminished; the emaciation very considerable; tongue morbidly clean; skin dry and scurfy. The cataracts are rapidly disappearing. Ordered to subsist exclusively on animal food, and to substitute the following medicines for those which the patient has been taking:

"℞ Sulph. zinci. ʒss. Extract. cinchonæ, ʒiss. ft. pil. xxiv. sumat ij. ter die c. haust. sequent.

"℞. Tinct. opii. gtt. vj. Decoct. cinchon. ℥j. Conf. aromat. ℈j. ft. haustus.

"March 3d.—Urine past the last two days, thirteen pints and a half—pulse 72—the cataracts have totally disappeared, and the patient's visual powers perfect; being enabled to employ her needle with perfect ease.—Continue medicines and diet.

"March 9th.—Urine has decreased to nine pints—pulse 94—the dyspnœa has very much abated, and there is a decided amendment in the patient's strength. Continue.

"March 14th.—Urine, eight pints—pulse 88.

"18th.—Urine 7 pints—pulse 96.

"25th.—Urine two pints, and natural in respect to colour and taste—pulse 80. The sole complaint appears to be extreme debility. The patient having regained her sight, and having been relieved of her diabetic symptoms, she became anxious to return home to her friends. She was accordingly discharged, with an injunction to come back to the hospital, should any of her former symptoms recur.

"May 10th.—Intense interest, as might naturally be expected, was excited with respect to the probable issue of this extraordinary case.

[illegible] lodging house, and [illegible] patient. From the [illegible] and has subsequently been [illegible] the patient.

[illegible] immediately (about [illegible] she began to experience [illegible] accompanied with great [illegible] that before a [illegible] To this [illegible] continued for several [illegible] per cent; but [illegible] was a complete restoration [illegible] more per [illegible] Her death took place [illegible] There had been no post-mortem examination.—Ed.

Case of Injury of the brain by Dr. Lyons. With remarks by Mr. Tyrrell.—A [illegible] on Saturday morning, July 10, [illegible]

He was [illegible] immediately after the accident [illegible] and was [illegible] Shortly after he became conscious, and was [illegible]

In [illegible] I [illegible] the assistance of three or four persons [illegible] to enable me to examine a wound of [illegible] extent on the forehead, just above the centre of the superciliary ridge. After enlarging the external wound I discovered an aperture, about the size of a crown piece, in the frontal bone, through which a considerable quantity of the substance of the brain was oozing, and as much as a tablespoonful adhered to the hair and integuments around.

Having fully exposed to view the nature of the injury, I gently carried my finger round the jagged edge of the bone, and introduced it

within the aperture, in order to ascertain whether any fragments of bone were lodged there; but as far as I could feel I did not discover any.

The wound was lightly dressed, and about sixteen ounces of blood were taken away from the arm, when he became more composed, and passed a tolerably quiet night. Both the eyes were closed from tumefaction of the palpebræ, and the face was also much bruised and swollen.

The next morning he was more tranquil, and appeared sensible when questions were put to him; the bowels were properly attended to, and saline medicines with antimony administered every fourth or sixth hour. Fomentations were applied to the face.

July 12.—Symptoms favourable, except the pulse, which, although not exceeding forty, was extremely tense and full. Fourteen or sixteen ounces of blood were therefore taken from the arm, and a purgative given. His nourishment consisted of tea and thin gruel.

July 13.—The pulse was somewhat quicker and softer. The dressings were removed altogether, and a soft poultice applied, except immediately over the wound, where a light dossil of lint with a pledget of cerate dressing was placed. The surface of the skin was moist and temperate, his sleep undisturbed, and he appeared quite rational and collected in his answers to the various questions put to him by the attendants.

July 14.—Went from this period until the 21st with little alteration in his symptoms, when, from fresh accession of febrile heat and irritation, it was judged expedient to repeat the bleeding from the arm to the amount of ten or twelve ounces; and, to counteract the stimulus excited by the heat of the atmosphere at this time, the room was kept as cool as possible, and his body and extremities were frequently sponged with cold vinegar and water.

July 22.—Passed a better night, and appeared altogether more comfortable. The wound was lightly dressed in the centre, and the surrounding parts continued to be fomented and poulticed three or four times in the course of the day: the fœtor from the wound was great, and the discharge very copious, but the surrounding soft parts had a healthy aspect.

July 25.—Symptoms generally favourable; no increase of heat, pulse tolerably soft, sleep almost natural, bowels regularly relieved; perfectly sensible, but the discharge from the cavity very abundant on each removal of the dressings.

July 27.—More pain in the head, bowels not relieved, some stiffness in the lower jaw, greater reluctance in answering questions.

July 28.—The bowels relieved by a purgative enema, stiffness of the jaw gone off, more collected.

July 29.—Able to take more nourishment than during the last two or three days, but still some sense of choking in the throat, and difficulty in swallowing; the discharge from the wound profuse; a little inclination to drowsiness, but nevertheless perfectly sensible; pulse about sixty, soft and regular.

August 1.—The difficulty of deglutition nearly subsided, and he was able to take a draught of port wine and water, broth, &c.

August 2.—The discharge continued very abundant, and the bodily strength apparently affected by it; but by improvement in diet, and giving pulv. cinchonæ ℈j every six hours, no other unpleasant symptom followed.

August 4.—Upon touching a point within the cavity, thinking it looked like a portion of bone, I was astonished that it resisted the probe like a harder substance than bone, and upon examining it with my finger I thought it felt like a portion of iron. I attempted to get hold of it with a pair of common forceps, but it repeatedly slipped from my grasp, and as it appeared to give the boy excessive pain (like drawing out his eye, as he expressed it) I desisted.

August 5.—I more distinctly found that the substance was iron, and that the projecting part of it resembled a screw. I now procured the assistance of four persons, in addition to that of Mr. Smith, a medical practitioner who attended with me, and after many efforts and considerable manual exertion, I succeeded in removing the substance from the head. I found it impossible to get sufficient hold of it to extract it by means of the forceps, I therefore made use of the common trephine elevator, with which I raised it from its bed, when the subsequent part of the operation was very easy. The body extracted proved to be the breach-pin of the gun, consisting of solid iron, three inches in length and exactly three ounces in weight, and of the form here delineated. I left him tolerably quiet and easy, betraying no additional unfavourable symptoms, the wound was dressed light and simply. The operation did not occasion any hæmorrhage.

August 7—For many days from this time no interruption occurred to his gradual improvement; but two or three small portions of bone came away with the dressings.

On the 16th he was carefully removed to his own home about a mile distant: he bore the removal without much fatigue.

August 25—Going on well, the cavity is gradually filled up, and the adjacent parts look healthy; he is able to sit up an hour or two in the day.

September 4—Remains in an improving state, free from any stupor, weight, or lightness of the head; is able to walk about a little in the house; sleeps well; bowels regular without the assistance of medicine; appetite good; and perfect recovery appears certain at no very distant period.

September 27—Very much improved; some more small pieces of bone have come away, and probably more will be thrown out before the wound closes.

November 3—The boy has continued in a favourable way, a few more small portions of bone have separated from the wound. He walks about, but is not allowed to exert himself in any way.

November 20—A further separation of bone has taken place, but yet not altogether sufficient to make up the quantity destroyed, more however appears forthcoming. The boy continues extremely well.

December 10—The boy is now in good health, no further exfoliation has taken place since the last report.

The following particulars have been obtained from the boy and from his mother, who has closely attended him throughout his confinmement from the accident.

The boy says that he remembers hearing the report of the gun when he shot at the crow; also that his little brother (who was with him at the time) walked with him into the yard, but that he was obliged to stop several times from faintness. He remembers his father coming to him in the yard.

Of what took place for the space of a week after his being taken into the house, he does not appear to have any recollection, although he certainly answered questions, put to him after the first day from the accident, correctly and consistently.

After the first week he readily knew persons who came to see him by their voices, when his eyes were perfectly closed from swelling of the palpebræ.

The sight of the left eye is totally destroyed, but that of the right remains perfect. The smell and hearing are both perfect, which his mother has had frequent opportunities of ascertaining, and she has not

found any defect in regard to his memory or general mental faculties since the accident. There does not appear to be any difference between the two sides of the face and head in point of sensation or power of motion.

Remarks on the Preceding Case, by F. Tyrrell, Esq. Surgeon to St. Thomas's Hospital.

Many cases are recorded in which large portions of the cerebrum have been lost without any immediate or subsequent derangement of the mental or corporeal functions; but I am not aware that any case has been attended with the lodgement of so large a foreign body in the cavity of the cranium, for so long a period as twenty-seven days, with the production of so little mischief.

In the first instance we may easily suppose that a portion of the cerebral mass, adequate to the bulk of the breach-pin, escaped from the aperture in the frontal bone immediately the injury was inflicted, and that thus the surrounding parts of the brain could not suffer from compression; but it appears strange that the violence which must have been necessary to force so large a body into the cranium should not have produced more concussion of the cerebral mass than seems to have taken place; perhaps the loss of blood soon after the injury may have prevented the appearance of more urgent symptoms.

The position in which the breach pin lay was with the screw to the aperture, and the other extremity towards the ear of the same side; the long limb was placed outwards, the short one inwards. From the length and situation of the breach-pin it appears certain therefore that both the anterior and middle lobes of the left side suffered; the former must have been very extensively lacerated at its anterior, inferior, and outer parts, the latter probably at its outer part only: thus the mischief appears fortunately to have been confined to parts of the cerebrum unconnected with any nerves essential to the support of the vital functions.

When we consider that not only this solid iron body, but that numerous irregular portions of bone were also lodged for so long a time in the substance of the brain and pressing upon its membranes, it does seem most extraordinary that the boy should have experienced so little inconvenience or suffering.

From the force necessary to extract the iron, I think it not improbable that the extremity of the long limb had penetrated the skull at the lateral part, and that it was thus fixed. Had it been perfectly loose in

the cerebral mass, I should imagine that its motion, when the boy changed the position of his head, must have occasioned great suffering, and have given rise to symptoms much more severe than those experienced.

The next extraordinary feature in this case is the trifling effect occasioned by the removal of the breach-pin; the surrounding parts must in a degree have accommodated themselves to its presence, and from a fresh disturbance I should have expected considerable irritation and inflammation to have followed. As the hæmorrhage produced by its removal was very trifling, it could not have had any effect in preventing unpleasant symptoms.—*Medico-Chirurgical Transactions, Vol. XIII.—part 2nd.*

5. *Poisoning with Arsenic.—Simultaneous poisoning of six persons. —Taste of Arsenic.—The Test of Reduction may supply evidence when the quantity of metal sublimed is less than a 250th part of a grain, and is too minute to form a characteristic crust.*—On the 1st November the family of a Baronet in Roxburghshire, together with several visitors, amounting altogether to six persons, all adults, were taken severely ill during dinner, or soon after it. Most of them rose from the table before the cloth was removed; but two of them were not seized till about an hour after the rest. The symptoms were in all sickness, vomiting, and severe pain of the bowels; and in no long time one of them had also diarrhœa. The vomiting continued violent till early next morning; and warm water was taken freely to encourage it. In the course of the night all were affected with a sense of heat in the stomach, throat, and mouth; and in the morning the lips became encrusted, and the skin racked and peeled off. For three or four days the whole party had a disinclination to eat.

The simultaneous occurrence of such symptoms, during a meal, in so many persons, left hardly a doubt that some poisonous substance had been mixed with one or other of the articles used at dinner. In order to discover what the poison was, and in what vehicle it had been taken, various articles were sent to me for analysis through Mr. Stuart of Kelso, one of the surgeons who attended the family.

It was at first imagined that the only article of which the whole party had partaken was soup, to which my attention was consequently directed in the first instance. In a general search for poison, it appeared to me that where no particular substance was pointed at, and the taste did not indicate an admixture of any of the vegetable poisons

which could cause symptoms like those specified above, the most natural substance to search for, and the only one which there was any probability of detecting, was one of the common metallic poisons. With this view, after filtering a portion of the soup, and acidulating it with acetic acid, I passed sulphuretted-hydrogen gas through it for fifteen minutes. But no change of appearance was induced. In this way it was rendered quite certain, that the soup did not contain any preparation of arsenic, mercury, copper, antimony, lead, or zinc. As the taste of the filtered soup was perfectly good, it was not easy to suppose that any other poison existed in it.

The next article examined was the vomited matter. Two quarts of this were sent me. It was taken from a pail which contained about four gallons,—the matter vomited by four of the party. A small portion of this when filtered gave no indication of any of the above metallic poisons on being treated with sulphuretted-hydrogen. But when the whole filtered fluid was evaporated to the volume of two ounces, sulphuretted-hydrogen produced a dirty yellowish cloudiness which, after ebullition and subsequent rest for twelve hours, gave place to a scanty, Naples-yellow, flaky precipitate. This precipitate was separated and washed by the process of subsidence and affusions repeatedly performed, and was then dried in a watch-glass. The product, which was very small in quantity, and of course contained a large proportion of animal matter, was then subjected to the process of reduction in one of the small tubes recommended by Berzelius. By slow and cautious management of the heat, a scanty sublimate was procured, forming a dark, filmy cloud, on a small part of the narrowest portion of the tube. This sublimate was entirely destitute of brilliancy on the outside, or of crystalline appearance on the interior; and consequently without the subsequent test of oxidation, suggested lately by Dr. Turner, no conclusion whatever could be drawn from it. But on removing the portion of the tube containing the flux, and then subjecting the film to repeated sublimation, a ring of fine sparkling white crystals was formed, on some of which I could observe with a common magnifier triangular facettes. I could hardly doubt, therefore, that I had procured a minute quantity of oxide of arsenic. But as the quantity was so small, that, being in the custom of weighing somewhat larger quantities, I was sure it did not amount to nearly a 250th part of a grain, I resolved to subject it to a farther test. With this view two drops of water were introduced into the tube, and boiled on the crystals.

These were soon dissolved, and in the solution the ammoniacal nitrate of silver caused as characteristic a lemon-yellow precipitate as it could produce in any arsenical solution.

The only obstacle in the way of the conclusion, that arsenic existed in the vomited matter, now was the possibility of a trace of it obtaining admission from the vessels employed in the analysis; for as these vessels consisted of four large glass funnels, as many precipitating glasses, and a porcelain evaporating basin, it might with some show of reason be said, that a few specks of arsenic scattered up and down these vessels, and too small to be detected by the sharpest eye, might be sufficient to make up the quantity procured. This consideration certainly led me to hesitate a little in pronouncing that arsenic had originally existed in the subject of analysis. At the same time there could be no doubt that the surface of the funnels and precipitating glasses, as well as the porcelain glaze of the evaporating basin, would, when clean and dry, enable one to detect the merest speck of adhering powder.

Fortunately all reason for hesitation was removed by the subsequent proceedings. It was afterwards remembered by the party that they had all partaken of certain bottles of wine, the remains of three of which were therefore sent to me. In a bottle of Teneriffe, and in another of a light French white wine, sulphuretted hydrogen gas caused no change whatever; and both of them were free of foreign taste. But in the remains of a bottle of Champaigne, which also was free of any taste except that of vinous sweetness, the same test caused a copious sulphur yellow precipitate. The arsenical nature of this precipitate was proved by the process of reduction. Two ounces of the wine gave one grain and a quarter of sulphuret of arsenic, corresponding to one grain of oxide of arsenic.

On subsequent inquiry, it appeared that the bottle of Champaigne was brought from the cellar before dinner by the baronet himself, who undid the wire during dinner, immediately before the wine was used. This circumstance at once showed that the wine had been poisoned before the bottle was corked by the wine-merchant.

Each of the six individuals took a single glass of it, or about two ounces; and consequently each took a grain of arsenic. It has been generally said in systematic works that a grain may prove dangerous. But I am not aware that any direct and positive fact has hitherto been published in support of this statement. The severe effects experienced by all the individuals in the present instance, notwithstanding that the

considerable quantity
single grain of arsenic
death itself.—Four
after drinking the wine,
that arsenic never
of the party perceived
and quite impartial tes-
of arsenic, concerning
have been published by
with other late
the fact, that arsenic may
in articles of food

the analysis of the vomited
chemists and physicians to the
test for arsenic. I
on this subject; but in
on the present
more than the process of
of the presence of this
form a character-
in *Edinburgh Med.*

Opium in mixed
—Mary White, aged 20,
the clinical wards
Dr. Alison, to
her symptoms.

ounces of laudanum at
About half-past ten
At eleven an emetic
of hot water; but
which was said to
she was brought to
following symptoms.

drooping; the
Sir answered
questions
was very anxious to

* See my *Treatise on Poisons*, lately published, p. 175.

be allowed to go to bed, and had the greatest inclination to fall asleep. She could swallow easily. The pulse was 86 and not strong, the tongue dry and brown, the respiration faint, the skin cold, and the general sensibility somewhat impaired. She complained of headach and giddiness, but had no pain in the pit of the stomach. The stomach-pump was immediately resorted to, and three basinfuls of water were injected and withdrawn. The first portions had the smell of laudanum. About five pounds contained in the first basin were subsequently subjected to analysis. After the stomach had been thoroughly cleared out, brisk cathartics, lemonade, and coffee were ordered, and she had the tepid affusion. At the same time directions were given to keep her in constant exercise in the green of the hospital. At four in the afternoon the pulse was only 48 and full. At seven the drowsiness had nearly gone off, and the skin was warm. The forced exercise was therefore discontinued. She slept quietly the whole succeeding night.

Next morning she complained of headach, and the face was flushed, and the pulse 84 and full. She had some vomiting and several scanty stools. There never was any itching of the skin, which M. Bally has lately considered an invariable symptom of poisoning with morphia and its compounds.* Purgatives were again prescribed, and the tepid affusion was repeated. In the evening, as her headach had increased, ten leeches were applied.

On the morning of the third day the headach was gone; but she complained of pain in the pit of the stomach, increased by pressure and coughing. The pulse was rather slow, the tongue furred, the bowels open, and the drowsiness not entirely removed. Purgatives were again ordered, and effervescing draughts given from time to time. Next morning, the 11th, she was quite well, and left the hospital.

Chemical Analysis.—The contents of the first basin, which had a distinct smell of opium when first withdrawn from the stomach, lost it almost entirely after exposure to the air for an hour. At least when I first examined the fluid after that interval, I was not sensible of any other odour, except the usual sourish, heavy smell of the natural contents of the stomach.

The fluid was colourless and nearly clear, and it contained only a few strings of mucus. When filtered, and cautiously evaporated to the consistence of a thin syrup, it was treated with boiling alcohol. A

* Mem. de la Soc. Roy. de Med. 1827.

large quantity of grayish flakes was thus thrown down, and the alcoholic fluid after filtration was brownish-red, clear, and distinctly bitter. This solution was in its turn evaporated to the consistence of a thin syrup, and then treated with distilled water. More insoluble matter was thus separated, and a fluid was procured of the same sensible properties.

This fluid was next precipitated by an excess of acetate of lead. After filtration and the subsequent removal of the excess of lead by a stream of sulphuretted-hydrogen, a very pale straw-coloured liquid was procured, which still retained a distinct bitter taste. This, when gently concentrated, precipitated with ammonia; and the precipitate formed slowly in tufts of radiated capillary crystals of a pale-brown colour. These crystals when cleaned with a little cold water, dissolved freely in boiling alcohol, and crystallized again after the alcohol had nearly all evaporated. They were so minute in quantity, that I could subject them to only one of the tests for morphia, nitric acid, which immediately dissolved them, forming a bright yellow fluid.

The insoluble matter thrown down by the acetate of lead was washed on the filter, and suspended in distilled water, through which a current of sulphuretted-hydrogen was transmitted. In this way the whole lead was thrown down in the form of sulphuret of lead, and all acids contained in the precipitate formed by the acetate of lead were disengaged and dissolved. With a view of discovering whether any meconic acid was thus separated, the solution was filtered and evaporated to the volume of half an ounce; upon which a faint cherry-red colour was struck in it by the perchloride of iron.

Observations.—My principal object in publishing the case now described is to show, in opposition to the expressed belief of some medical jurists of authority, that even in circumstances apparently very favourable, the analyst may fail to procure satisfactory indications of the presence of opium in cases of poisoning with that drug.

In this instance two ounces of laudanum, a quantity rarely exceeded in cases of poisoning, was retained only four hours in the stomach. Very little could have been discharged before the stomach-pump was used, as appeared from the want of colour of the small quantity of matter previously vomited. To the first portions of the contents of the stomach I applied the process recommended in my Treatise on Poisons,—a process which I have found to be one of much delicacy, and which is derived in part from the instructions I received from M. Robi-

quet of Paris, when I was his pupil, and partly from an analogous method devised by *Lassaigne* for detecting acetate of morphia. Nevertheless, I was unable to develope any of the characters of morphia except its bitterness, its precipitation from a state of solution by ammonia, and the imperfect action of nitric acid, which with pure morphia produces an intense orange-red solution; and the only proof which could be procured of the presence of meconic acid was the imperfect action of the perchloride of iron, which with even a very minute trace of pure meconic acid occasions in the solution an intense cherry-red colour.

Supposing the preceding case to have been a criminal one, would these facts have constituted sufficient evidence of the presence of opium? I must confess I have some doubts on the subject. If however, I had been quite satisfied of the opiate odour having existed in the fluid drawn from the stomach, my mind would have been made up; but no odour of the kind was perceptible to my sense of smell an hour after the contents were withdrawn: and the sense of smell is so apt to lead to error, that no sound inference can be drawn from it, unless several people agree in opinion as to the odour. The circumstances of the case show the propriety of applying the test of the odour immediately after the contents of the stomach have been removed.

It is hardly necessary to add, that the failure of the chemical analysis in such cases is owing not so much to imperfections in the process, as to the rapid absorption and decomposition of the poison in the stomach.

The present is not the only occasion on which my attempts to discover opium, in cases of poisoning with it, have failed. But it is unnecessary to state the particulars of any others, as they present no peculiarity. The subject is now becoming one of very great importance in medical jurisprudence. Many examples of poisoning with this drug have occurred where it was given for criminal purposes, either with the direct intention to murder, or for the purpose of causing stupor previous to the commission of robbery; and since the recent trial of Stewart and his wife before the Justiciary Court here for these crimes, the latter crime has become apparently much more frequent than ever. It is of great consequence, then, either that a delicate and certain process be devised, or at all events that the medical jurist know the exact extent of his powers.—*Ibid.*

7. *Portion of Bone lodged for forty eight hours in the Trachea of an Infant. In the London Med. Gazette*, No. 105, Mr. Thomas Stabb relates, that on the 20th of September, 1829, his child, aged ten months, detached a small portion of bone about the size of a large marrow fat pea, from a bone of a neck of mutton. It slipped into her windpipe, and produced violent coughing for about five minutes, when it ceased, leaving a noise in breathing like that produced by a saw. In twenty-four hours there was much irritation and fever, with difficulty of breathing, which were subdued by the usual remedies. After the fourth day, the child seemed as well as usual, except this constant wheezing. On the 3d of November, six weeks after the accident, violent cough and irritation supervened from exposure to cold; antimony was given, and on the 7th, after a dose which produced vomiting and general relaxation, and while the nurse was briskly rubbing the throat with a volatile embrocation, the head being bent back over her lap, she was seized with a violent fit of coughing, and threw up the piece of bone embodied in mucus, which had been retained forty-eight days in the trachea; her breathing almost immediately became natural, and the next day she was as well as ever. The piece of bone was very rough, of a triangular shape, and the edges quite sharp.

OBITUARY NOTICE.

Died, in the month of March last, at the house of his father in Winchester, Virginia, Stephens Rittenhouse Beattie M. D.

Dr. Beattie was well known to a number of the subscribers to this journal, during his attendance on the medical lectures of 1827-8 in this University. He graduated in the spring of 1828 with distinguished credit. His conduct toward those with whom he associated was such as to secure their esteem in a very high degree; and he left behind him in Lexington, with Professors and Students, the reputation of a man of talents and worth. He was distinguished for his anatomical knowledge, which was the foundation of the extraordinary skill he displayed in some operations of great difficulty. He was particularly successful in the treatment of some very bad cases with the bandage as applied by Professor Dudley. Natural talents, acquired knowledge and great promptitude and decision in the application of it, soon raised him to a high standing in his place of residence, Clear Spring, Washington county, Maryland, and furnished him at an early period with abundant means of living comfortably and of prosecuting his studies to great advantage. Unfortunately however he became dissatisfied with a residence in a village, gave up a lucrative and increasing practice, and went to Rochester in New York, and thence, his expectations there not being realized, to Washington City, to reside. His health was not the most firm from his early youth, and during the prosecution of his medical studies, he suffered severely from dyspepsia. This he endeavoured to relieve by adopting the treatment recommended by Dr. Johnson, but without success. Abstinence carried to a great length produced excessive emaciation, and the state of his health conspiring probably with disappointed expectations, induced an apprehension of bad consequences from the further prosecution of the plan of abstemious living, and led him to adopt one of an opposite character. The sudden change brought on mania, and although the violence of the disease is said to have been reduced, in a moment of distraction he destroyed himself by cutting the femoral artery with a razor as he lay in bed.

TRANSYLVANIA UNIVERSITY,

MEDICAL DEPARTMENT.

At a public commencement held in the Medical Hall, on Wednesday the 17th of March 1830, the degree of *Doctor of Medicine* was conferred upon the following gentlemen, who submitted Dissertations on the subjects annexed to their names: viz.

I. FROM KENTUCKY.

1. Jonathan J. Bradford—on Inflammation.
2. Benjamin Bridges—on Dyspepsia.
3. John Browne—on Fever.
4. Owen W. Bush—on the Diagnosis and Surgical Treatment of Aneurism.
5. Dabney Carr—on Curved Spine.
6. Elijah Carroll—on Hernia of the Abdominal Rings.
7. Joseph J. Clark—on the Constitutional treatment of local Diseases.
8. Samuel H. B. Clarkson—on the Unity of Fever, and the functions of the Liver.
9. Benjamin P. Drake—on Fever.
10. Frederick G. Gilmer—on Menstruation.
11. Meriwether L. Gilmer—on the Podophyllum Peltatum.
12. Albert B. Graham—on Hydrocele.
13. John W. Henry—on Paronychia.
14. John L. Mothershead—on Peripneumonia.
15. Benjamin W. Sinclair—on Menstruation.
16. John F. Tomlinson.—on the Modus Operandi of marsh miasmata in the production of Disease.
17. Nathaniel W. Vallandicham—on Cholera Morbus,
18. James Wilson—on Prolapsus Uteri.

II. FROM TENNESSEE.

1. Christopher H. Dabbs—on Sedatives.
2. Thomas J. Donoho—on Milk-Sickness.
3. Milton P. Ewing—on Bilious continued Fever.
4. Brice M. Hughes—on Digestion.
5. Harrison Jordan—on Typhus Fever.
6. George Keeling—on Dysentery.
7. Thomas J. Kennedy—on Sedatives.
8. Patrick D. Neilson—on Blood-letting.
9. Charles B. Raines—on Milk-Sickness.
10. Elijah Thompson—on Hysteria.
11. Gideon B. Thompson—on Chlorosis.
12. Peter B. Whiting—on the Papaver Somniferum.

III. FROM SOUTH CAROLINA.

1. James Burns—on the Pathology of Hæmorrhage.
2. Beverly T. Coalter—on Dyspepsia.
3. James B. Davis—on Oleum Teribinthinæ.
4. Mark M. Johnson—on the Proximate cause of Inflammation.
5. William Ligon—on Inflammation.
6. Wiley K. D. Lindsey.—on Menstruation.
7. Josiah Montgomery—on Icterus Infantum.
8. Jesse C. Patrick—De Sanguinis Circulatione.
9. George G. Perrin—on Cynanche Tonsillaris.
10. Joseph A. Reese—on Syphilis.
11. Robert W. Woods—on Dropsy.

IV. FROM VIRGINIA.

1. Edward H. Allen—on Bilious Inflammatory Fever.
2. Frederick A. Davisson—on the Proximate cause of Dropsical accumulations.

3. Samuel Hatch—on Blood-letting.
4. Edward Lee—on Scrofula.
5. David G. Smith.—on Bilious Pleurisy.
6. Irwin Stith—on Menorrhagia.
7. Abner Y. Watson—on Gastro-enteric fever.
8. Henry H. Withers—on Dysentery.

V. FROM ALABAMA.

1. Jordan Y. Cummings—on the uses of the Bandage.
2. Thomas S. Early.—on the History, Medical properties, &c. of the Secale Cornutum.
3. William C. English—on Chronic Dysentery.
4. James Moore—on Emmenagogues.
5. John K. Nelson—on Rheumatism.
6. Thomas C. Nelson—on the efficacy of Emetics in Mercurial Salivation.
7. Francis M. Phillips—on Syphilis.
8. Hartwell V. Smith—on Dysentery.

VI. FROM GEORGIA.

1. Joseph W. Anthony—on Cynanche Trachealis.
2. James Brawner—on the Medical Topography and endemic Bilious Fever of Elbert county, Georgia, as it occurred there in 1829.
3. Thomas H. Chivers—on Intermittent and Remittent Bilious Fever.
4. James M. Cunningham—on the use of Mercurial Cathartics in Bilious Fever.
5. Aurelius N. Jones—on Malignant Ulcer.
6. Samuel B. McNees—on Typhus Fever.
7. Ellis B. Robinson—on Puerperal Fever.
8. Martin G. Slaughter—on Pleuritis.

VII. FROM NORTH CAROLINA.

1. JOSEPH W. HALL—on Congestion of the Liver.
2. GEORGE HOKE—on Scrophula.
3. JAMES H. TATE— on the Nature and Treatment of Blenorrhœa.

VIII. FROM MISSISSIPPI.

1. JAMES LYNE—on Dyspepsia.
2. WILLIAM W. WORTHINGTON—on the Modus Operandi of Medicines.

IX. FROM MARYLAND.

1. JAMES G. SNYDER—on Tetanus.

The *Honorary degree* of Doctor of Medicine was also conferred upon SAMUEL GRANT MITCHELL, of Indianapolis, Indiana.

C. W. SHORT M. D. DEAN.

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE,

AND THE ASSOCIATE SCIENCES.

AUGUST, 1830.

ORIGINAL COMMUNICATIONS.

ARTICLE. I.—*A Valedictory Address, on some of the duties and qualifications of a Physician, delivered to the Graduates of the Medical Department of Transylvania University, on the 17th day of March*, 1830. By CHARLES CALDWELL, M. D.

[PUBLISHED BY REQUEST OF THE GRADUATES.]

GENTLEMEN,

NOTHING is more true and worthy of consideration, than that a single error in words, proves, not unfrequently, an abundant source of error in practice. This is more especially the case when the expression conveying it is comprehensive in its meaning, and general in its tendency, and is so often repeated as to pass into a proverb. And the evil is increased in proportion to its connection with motives to action. Under these circumstances the error takes deep root in the mind of the public, becomes amalgamated with their feelings, influences their conduct, and eventuates in mischief.

Of this description is the sentiment conveyed in the phrase,

whose currency is universal, that some of the youth of our country have "finished their education." Nor are there in our language many expressions fraught with more of sweetness to the lips of those who utter them, or of music to the ears of those to whom they are addressed. The consummation which the phrase announces is an event composed of delightful elements. To present joys it unites gratitude and endearing recollections of the past, and the fascinating creations of hope for the future. While the retrospect is rich in well remembered forms of realized pleasure, the prospect discloses a world of promise.

When, at the age of fifteen, a female, having learnt somewhat of the common rudiments of knowledge, and made a limited progress in fashionable attainments, takes leave of a boarding-school, and returns, in raptures, to the home of her childhood, she is congratulated by her friends on having "finished her education." The youth of our own sex is warmly greeted on having "finished his education," when, at the age of eighteen, after having learnt a little Latin and Greek, and conned a vocabulary of technical phrases, he receives the degree of Bachelor of Arts, from some institution authorized to confer it. The candidate for forensic honors is met by salutations on having "finished his education," as soon as he is admitted to a seat at the bar. And the pupil of the healing art is felicitated on the same event, the moment he is clothed in the honors of the Doctorate.

Such, I say, are the greetings which welcome our youth from the toils of study, and the discipline of the schools. Nor could stoicism itself either condemn the motives which prompt to them, or remain unmoved by the scenes they present. In addition to being intended as encouragement in well-doing, they are the offerings of kindness on the altar of friendship, and the outpourings of the heart, in a moment of feeling, dispensing instinctively the richest reward, that affection can either receive or bestow.

But delightful as they are to those who are the objects of them, and laudable as is the intention from which they proceed, far from being in all cases salutary, they are not even innocent in

their effects. Like the voice of the Siren, they are calculated to deceive; and the draught which accompanies them not only intoxicates; to many who quaff it, it is drugged with poison. The individuals referred to have not finished their education. The declaration to that effect is a compound of ignorance and error, and ought not to be addressed to them. To their heated imaginations it presents a phantom, not a reality to their temperate judgment. Nor is this the worst. It pampers their pride, inflates their vanity, gives them false and extravagant notions of their attainments, and too often leads to neglect and indolence. For why should those whose "education is finished." toil any longer in search of knowledge? Such is the language of their self-love.

It is not true that the female has nothing further to learn, when she bids adieu to the boarding-school, or the youth when he issues from the hall of the University. And the same may be asserted of those who are just graduated in medicine and law. I will not say that individuals so immature have yet every thing to learn; but I do say that they have yet much to learn, the neglect of which would render them almost as useless as if they had never learnt any thing. Their education, therefore, I repeat, is not finished. To the truth of this all competent judges will testify. Shall I be asked, who are competent judges? I answer, those alone who have themselves attained to eminence in knowledge.

As relates to females, then, inquire of a Montague, a De Stael, an Edgeworth, or some of our own distinguished countrywomen, whether their education was finished, when they took leave of their boarding-schools? Ask a Porson and a Parr, whether their education was finished, with the close of their pupillage in Oxford and Cambridge? Of a Boerhaave, a Haller, a Cullen, and a Rush inquire, whether their education was finished, when the Doctorate was awarded them? And to an Erskine, a Pinckney, and an Emmett propose a similar question, as relates to the law? To each interrogatory the answer will be the same—an emphatical No. The individuals questioned will even add, that, far from being finished, at the periods specified, their education was but begun; that the foundation was

indeed laid, but the entire superstructure was yet to be erected. Inquire of them again, by whom the edifice was ultimately completed? and they will answer, that it was afterwards incessantly laboured on by themselves. Dropping the metaphor, they will modestly subjoin, "during a long pupillage of industry and exertion, all we could do, was to acquire some portion of the elements of knowledge, with an improved fitness for the attainment of more. But instead of being finished in *youth*, we have found education to be the work of a lifetime, and it is still incomplete." Nor, consistently with truth, could any one return a different answer to the interrogatories proposed.

If, then, such is the case, as relates to other physicians, some of them as eminent as the world has produced, it is equally so with respect to yourselves. You have not yet attained to maturity. True, you are clothed in the honors of your profession; and the guerdon has not been lightly bestowed. By your conduct and attainments you have abundantly deserved it; and long and prosperously may you live to enjoy it. May your career be such—and my present feelings do not suffer me to doubt that it will—as to throw back hereafter, with augmented lustre, much of the honors, with which you have been invested, on the institution that has bestowed them!

But, while, in all sincerity, I thus testify to your merit, the same sincerity compels me to add, that your education is not finished. The foundation of the fabric is laid, I trust so enduringly, that, instead of impairing, time will only cement and strengthen it. But by your own persevering industry and labour must the edifice be reared. Do you ask me, in what way the work may be best accomplished? The answer requires a little detail. Permit me, therefore, to devote the remainder of the period, which the occasion allows me, to an attempt to render it.

Your education, which has been so successfully conducted to the present point, you must prosecute yourselves, by a fourfold process; reading, observation, reflection, and writing; and they should be mingled in such proportion, and so alternated, that each may operate in aid of the others. My remarks, in il-

lustration, under the several heads, into which this proposition divides itself, must be necessarily brief.

It is probable that most of you will be chiefly engaged in country practice; and there is a prevailing belief (perhaps I should call it a pretence) that the labours of that, and the time consumed in it, are incompatible with reading. But, whether it be belief or pretence, it is a mistake. Nor is this the worst view of the subject. It is alleged, in most instances, as an excuse for idleness. Those who affect to have no leisure for the cultivation of science, have an abundance to bestow on the perusal of newspapers, on general gossip, and on the reading of political and theological pamphlets, which, without informing the understanding or improving the morals, inflame the passions, excite discord among neighbours, and interrupt the tranquillity of the country. That this is true, is susceptible of proof. It is within my own knowledge, that many physicians, engaged in a full and lucrative country practice, read extensively, and to great advantage. The reason is obvious. Determined to read, they divide and appropriate their time judiciously, and therefore succeed. Take this as a maxim of high value. Whatever man resolves and strenuously endeavours to do, he *can* do, provided he add judgment and perseverance to industry. By the condition imposed his aim must be within the bounds of reasonable ambition. Let this govern your conduct, and your failures will be rare. It is applicable to all the pursuits of life.

There are few, if any, practitioners of medicine, who cannot, on an average, by economizing their time, read attentively twenty pages every day. This will make an aggregate, which, in round numbers, may be stated at seven thousand pages a year. That will amount to nearly a dozen and a half of large volumes, which, if skilfully selected, will contain all the annual discoveries and improvements in medicine. It is in the power, then, of every physician, easily to keep pace with the progress of his profession. But, to enable him to do this with greater certainty, and less difficulty, he ought to read regularly two or three well conducted medical Journals. If he neglect this, something which he should know will inevitably escape him.

Experience proves, that the best mode to acquire a respectable stock of knowledge, by limited reading, is to be familiar with periodicals.

But, although books, which are the product of the pen and the press, will impart to you an abundance of useful knowledge, they will not alone be sufficient for your improvement. They certainly will not conduct you to eminence. If you be true to yourselves, your profession, and the public, the most invaluable source of your future progress in science will be the book of nature. That is a production whose stores are inexhaustible, and which will never deceive you, conditioned that you learn to interpret it correctly. Its author is the Deity, the living fountain of truth and wisdom, and at once the source and essence of perfection. With his own hand he traced the mighty work, condensed in it all that intelligence can grasp, and spread it through creation for universal instruction. In plainer language, it is creation itself. This volume is free from errors, unpolluted by interpolations, and bears on every page the impress of Divinity. Nor does it possess more of truth than of elegance and splendour. And its boundless variety will bear comparison with its other excellencies. All written works are valuable only in proportion as they are faithful translations of it, or pertinent commentaries on it. It is the only correct standard of their genuineness; the test by which alone their sterling can be tried. All that is opposed to it, and every deviation from it should be condemned and rejected. Never neglect, therefore, to compare with its contents whatever you may find in the productions of the press. And let your comparison be severe, that the result may be certain. If the concordance be obvious, believe and adopt; if doubtful, suspend your belief, and continue to examine; but if a real discrepance exist, the work, I say, is spurious, and unworthy of your confidence.

Do you ask me, how the book of nature is to be studied and mastered? I answer, by the natural processes of observation and reflection. The characters in which it is written are intelligible to all, who carefully examine them; and its contents consist of objects, events, and their relations. The knowledge

of the two former you will attain by observation, and of the latter by reflection. Nor can it ever be attained in any other way. It is by these two processes that you can truly teach yourselves, and advance in science with independence and confidence. Reading alone will give you learning and information; but it will habituate you too much to rely on others, and will not completely develop your strength. It is but the crutch to him who fears to walk without it. Nor can you, by means of it, improve your profession, an end at which you should resolutely aim. Unite to it observation and reflection, and they will render you vigorous and independent, provident and wise; fertile in resources, and prompt and judicious in the application of them. It is only by the study of nature that you can add to the existing stock of medical knowledge, and, as the result of your own labours, leave your profession better than you found it. Much is said of "self-taught" men, and high commendations are frequently bestowed on them. But so common is self-teaching, that such individuals do not constitute a distinct class. Every one, who is thoroughly taught, is, in a great measure, "self-taught;" and it is principally by observation and reflection that the work is performed. Scholastic teaching is valuable chiefly, not on account of the sum of knowledge which it actually imparts. That is necessarily limited. But it awakens ambition, improves the taste, disciplines the mind to regular action, indicates the course that most directly leads to knowledge, and thus facilitates the process of self-teaching. The man who is not thus instructed might almost as well be uninstructed. All you can receive from others, whether orally or in writing, will avail you but little, unless you assimilate it and make it your own. And this you can do in no other way, than by observation and reflection. These processes are to the intellect what those of nutrition and muscular motion are to the body. They bestow on it substance, activity and strength. When you are merely imbibing knowledge from others, whether by reading or listening, your intellects are comparatively in a state of repose. Nor can their passive condition, as mere recipients, ever invigorate them. But, under the exercise of observing and re-

flecting, the case will be different. All the strength and efficiency of which they are susceptible will thus be developed. The individual, moreover, who only reads and listens, can never be more than a mere copyist, or at best a borrower. But he who observes and reflects may become an original. Under this head I shall only add, that the process of acquiring knowledge by observation is an art; and that practice alone is the source of dexterity in it. Nor is any one aware of the adroitness he can attain in it, until taught by experience. Even an unlettered individual, well versed in observation, can collect, by means of it, in a given time, a much greater amount of knowledge, than the ripest scholar can acquire, by the same process, if he has not practised it. Nor is there any character, whose opportunities to improve, by observation and reflection, are better than those of a physician in the country. Notwithstanding the sentiment which prevails to the contrary, he has it in his power to devote a large portion of his time to the cultivation of his intellect. In travelling from house to house, in the practice of his profession, he may be considered alone in his study, with no intruder to interrupt his thoughts, while both past occurrences and present objects, if observed and reflected on, are full of instruction.

But so extensive and multifarious is the book of nature, that it will be impossible for you to study it throughout. Should your lives be protracted to antediluvian longevity, and not a moment of them be wasted in idleness or devoted to relaxation, the period would be too short for a task so unbounded. Be it your business, then, to select such portions of it, as may be more immediately subservient to your professional improvement, and to render yourselves thoroughly masters of them. Of that great work, which is always and every where spread before us; by day and by night; on the land, in the water, and through the heavens; on the mountain and the hill top, in the valley and the plain; during the freshness of spring, the luxuriance of summer, the mellow richness of autumn, and even the cheerless sterility of winter; and which meets the eye alike in the saloon of health, the chamber of sickness, and the house of death; of

that work, which, as the source of knowledge, is "all in all" to the votaries of truth, permit me to point out a few chapters, which are more peculiarly worthy of your attention.

The first I shall refer to is that on *Topography* and *Climate.* These are agents of no common power in modifying the human constitution in health, and in giving peculiarity to disease. To become accomplished in the knowledge of his profession, therefore, and competent to all its practical duties, every physician should study them accurately in the place where he resides. They include soil, water, and other minerals, native vegetable and animal productions, atmospherical temperature, vicissitudes, humidity, and dryness, cloudy and sun-shiny weather, prevailing winds, and the frequency and amount of frost, rain, hail, and snow. Add the mechanical structure of the country, and the catalogue will be sufficiently full for your purpose.

These, I say, are causes of no small influence in the modification of disease. To this truth all experience testifies. The complaints of the north and the south, and of the summer, winter and spring, are different from each other. So are those of mountainous, hilly, and level regions. Nor are the diseases where the soil is loamy and fertile identical with those where it is sandy and sterile. The complaints of humid situations differ also from the complaints of dry ones. It might seem superfluous to add, that, under all such modifications, their danger is not the same, and that their treatment must be different. The study of these topics, then, which are expounded most satisfactorily, if not alone, in the book of nature, will afford you agreeable and profitable employment; and the knowledge of them will add to your standing and usefulness. It is understood, as a matter of course, that, in pursuing such inquiries, the connexion of disease with the various subjects of them will receive attention. In a particular manner, are there any noxious vegetables, which derange the human system, by acting on it directly, or by means of the tainted milk or flesh of inferior animals, domestic or wild? If so, what are they, and how may they be destroyed, or their influence best counteracted or prevented?

Of the chapter which treats of *comparative* anatomy and

physiology I cannot speak too highly. It is one of the most interesting and instructive in the work. And its matter is essential to the educated physician. Your attention to it, therefore, should be measured by its importance. Disease is but derangement of structure and function. Without a competent knowledge of the two latter, then, in a healthy and natural condition, the former can be neither understood nor successfully treated. To be qualified to discover whether structure and function are deranged, and if so, in what way and degree, you must have a perfect acquaintance with them when they are not deranged. But to become thoroughly versed in physiology, in particular, by studying the human system alone, or even the systems of the higher orders of animals, is impossible. There exists between all kinds of living matter many points of striking and very instructive analogy. The economy of one kind throws light on that of another, as the heavenly bodies are mutually illuminated by an interchange of rays. It is only in the systems of living beings of the lower and simpler forms, that the *true elements* of physiology exist. At least, it is there that they are most easily comprehended. It is systems void of complexity, where tissues are insulated, and the action of one organ is neither obscured nor modified by that of another, that may be said to constitute the alphabet of the science. But the alphabet must be learnt antecedently to reading. An acquaintance with the functions even of vegetables is essential to a satisfactory knowledge of the functions of our own bodies. It is only by the aid of comparative anatomy and physiology in particular, that the human brain can be fully comprehended in its complex character and multiplicity of powers. He, then, who would thoroughly understand the economy of any one kind of living matter, must study that of many kinds. So true is this, that the individual who is not a comparative physiologist, has no claim to the standing or even the name of a physiologist at all. His views must be, in *every* case, limited, and, in many, erroneous. He is, in science, a sectarian; a term which denotes devotion to dogmatism, and hostility to liberal and catholic research.

Another chapter of great value, is that whose subject is *Pathological Anatomy.* An intimate acquaintance with the seat, appearance, and nature of the læsions that constitute the immediate cause of disease, is essential to him who aspires to be a philosophical physician, and a successful practitioner. It is that which forms one of the broadest lines of distinction between scientific and empirical medicine. It is, moreover, a point of striking contrast between the state of our profession now, and that which existed even at the commencement of the present century. But little attended to then, pathological anatomy is an object now of universal attention. Being an improvement of peculiar moment, it shows that medicine is rapidly advancing. To keep pace, then, with the science, it is indispensable that you cultivate it. Neglect no opportunity that may offer, to ascertain, by examination, the seat of disease, the nature of the læsion, and the cause of death. Thus will you turn your experience to the best account, and convert a failure, in one case, into the ground of success in another. Such is the high work of wisdom, in all the walks of human life; to take lessons from experience, to learn truth from mistake, and to profit by misfortune. The physician who thus instructs himself, by every occurrence, whether prosperous or adverse, will do much good, and acquire reputation. And he owes the issue to observation and reflection. But he who neglects such opportunities, or knows not how to turn them to advantage, will neither attain professional standing nor deserve it.

A fourth very interesting chapter is that which relates to epidemic diseases, their history, philosophy, and laws. Whence do they come, and what are their causes? What relations do they bear to season, situation, and the sensible qualities of the atmosphere? What to the clearing and cultivation of the country? What description of subjects are most liable to them? and to what description are they most dangerous? Are they propagated by contagion, as well as by atmospherical influence? Whether do they occur irregularly or at stated periods? and are their type and character always the same, or do they differ so widely at different periods, and in different seasons, as to call

for different modes of treatment? Do they attack any of the inferior animals, in common with man? Do epidemics follow each other in trains, and a stated order? Are they always or mostly accompanied by unusual meteorological phenomena? Can two epidemics co-exist in the same place? Respecting some of these topics but little is known, and different opinions are entertained with regard to others. Further observation and study can alone throw light on them, and entitle our views of them to the name of science. To most endemic diseases similar remarks may be correctly applied. Their dependence on place and season is a subject of inquiry peculiarly interesting. A strict and steady attention by you to the several points here enumerated, cannot fail to eventuate advantageously to yourselves and others. It will enrich medicine, give you standing with your contemporaries, and aid in holding you up as lights to posterity.

A fifth chapter you will examine for a knowledge of *marsh miasmata.* The subject is important, and we have much respecting it yet to learn. The following are some of the points to be investigated. Does the poison arise exclusively from decaying vegetable, or from a mixture of animal and vegetable, substances? What are its composition and nature, and how may it be destroyed? To what distance from its source does it travel over land, and to what distance over water, and produce disease? To what elevation does it rise in the atmosphere? Whether is it most dangerous during the day or night, to those who are exposed to it? If during the night, at what particular hours, and for what reason? What are the best means to prevent its production, and to guard habitations from its influence, when it is produced? Are more than one kind of febrile miasms generated by the decay of organic substances? and if so, what are the differences between the processes from which they arise? On what part of the system does marsh miasm primarily act in producing disease?—Conclusive answers to these questions are important desiderata in the science of medicine. And they must be the fruit of future observation and reflection. I shall only add, that those whose fortune it may be to succeed in

rendering them, will confer on mankind unspeakable benefits, and immortalize themselves.

A sixth chapter contains the history and philosophy of man, as a physical, organic, moral, and intellectual being, with all his relations in those capacities. I need scarcely remark to you that this portion of the volume is replete with matter of infinite moment. And such is its nature that it should be known not only to every educated physician, but to every one who pretends to science, and who may be called on to instruct, direct, or command, or to exercise any function, that may lead him into an extensive intercourse with his race. It should be known, in a particular manner, to the lawyer, the jurist, the statesman, the diplomatist, and the divine, else they are unfit for their respective stations. It is from this chapter we learn that, on earth, man enjoys a situation, and is in the midst of a general condition of things, adapted to his nature; that he is formed in harmony with all things around him; that if he preserve this harmony, which he is competent to do, he will be comfortable and happy; but, that if he interrupt it, he will suffer. We further learn, that, being usually the author of his own misfortunes, he has no right to complain of the hardships of his lot, or to murmur on account of the miseries he endures. He has deserved and invited them, by violating some of the relations of his nature. Had he made himself well acquainted with those relations, and acted in conformity to them, evil would not have befallen him. The dispensation under which he lives, therefore, is one of right and justice. The principles which regulate his movements make a part of his constitution, and are as stable and well defined, as those that govern the movements of the planets. Hence they are, in all respects, adapted to his nature. Were not this the case, they would not be binding on him, except by a mandate of arbitrary power. But, in the administration of the universe, no such mandate is ever issued. Throughout the whole of it, from the highest objects and concerns to the lowest; from men to mites, and from worlds to atoms; from the subversion of kingdoms to the fall of a sparrow, every thing is governed by its own stated laws, and not by accident or arbitrary

sway. While these laws are obeyed, all things go right. But, every departure from them produces, of necessity, confusion and mischief. And this is as true of the organic, moral, and intellectual departments, as of the physical; as true of ourselves as of dead matter.

Does man infringe a physical law? He suffers, in some way, by physical force. Does he violate or neglect an organic law? As a direct consequence, his health is deranged. Does he trespass on a moral law? He suffers in his reputation and interest, or in his own estimation and the tranquillity of his mind. His standing and business are injured, or his conscience is disturbed. Does he do violence to a law of his intellectual nature? He suffers the penalty of ignorance, by failing in some enterprise in which he has engaged, without the knowledge essential to success. To illustrate and prove each of these points, by daily and familiar occurrences, would be easy. But time does not permit me to dwell on the subject.

It is in this portion of the volume that you will read correct lessons on that interesting topic, the human temperaments. In examining it you will perceive that those peculiarities in the constitution of man arise from the solids of his body, not, as is generally believed, from the fluids. You will learn that they are the result of a preponderance of some of the ruling organs of the system, giving character to the whole. And you will further learn, that this preponderance of organs in influence, is the necessary result of their preponderance in size. It is already known to you that the controling organs of the body are those contained in the three great cavities of the cranium, the thorax, and the abdomen. According, then, as the brain, the lungs and heart, and the chylopoetic viscera are well balanced, or preponderate, in size and strength, the one over the others, is the temperament of the individual. The subject should be carefully studied by every one, who is ambitious of a knowledge of the human character.

In earnestly recommending this chapter to your attention, I feel authorized to say of it, that it contains matter amply sufficient, and peculiarly calculated, to "vindicate the ways of God

to man." I shall only add, that it is the chapter on Phrenology; and that the views of human nature just presented, are the fruit of that science. It is by the study of it, in aid of other and higher sources, that you can ever understand the true relations between man and the objects around him, whether animate or inanimate, and especially those between him and his God. Instead, therefore, of being, as many unknowingly pronounce it, unfriendly to religion, it is an auxiliary and philosophical exposition of its real foundation.

Other chapters in the book of nature both agreeable and instructive, are those which treat of mineralogy and geology, botany and natural history. And with these every physician should be, in some measure, acquainted. His obligation to this effect arises from various considerations. Not only will he become, by the attainment, a more enlightened and an abler physician; he will fill a wider sphere of general usefulness, and sustain a higher rank in society. But to this he is urged, as well by motives of self-respect, as by those of professional ambition. He should not suffer himself to be surpassed in the knowledge of nature, by men who are devoted to other pursuits. From its derivation, the term physician means an individual skilled in nature. The physician, then, who is not so, is untrue to his title, and therefore unworthy of it. Nor is this all. He is unfaithful to his profession, in not contributing his part to sustain it in its ancient and well earned renown. Search its history to its earliest annals, and you will find it at all times, and in every country, greatly in advance of other professions, in the knowledge of nature. Such was the case in ancient Egypt, Greece, and Rome; and but for the faint lights which it shed around it, during the dark ages, the science of nature would then have been extinct. Throughout that period the subtleties of metaphysicians, the mysteries of the church, and the dreams of the cloister were not only departures from the simple truths of natural science, but in deadly hostility to them. Look into later times, and you will find, that a vast majority of mineralogists, geologists, botanists, and naturalists, who, by the extent of their contributions to science, have done honour to their countries, and benefited

their race, have been physicians. But there exists another reason why the physicians of the west should devote themselves faithfully to the study of nature. The valley of the Mississippi is but very partially explored. The knowledge of its mineralogy, geology, botany, and natural history is in a corresponding degree defective. To those who may engage in the study of them, therefore, these branches of science open prospects peculiarly inviting. Added to great usefulness, they promise the meed of lasting reputation.

One chapter more shall close my selections from the book of nature. And it is beyond all others resplendent in beauty and exquisite in delight. It is the chapter on poetry. But I must not attempt to analyze it, for a twofold reason. Time will not permit me; and none but a poet is competent to the task. The inspiration and conceptions suitable to the effort are not mine. Glowing, lofty, and redolent of divinity, they belong only to the Homers, Miltons, and Byrons of our race. Much is said of the "*creations* of poetry," as if the product of that kind of writing were not a picture of things as they are; as if it consisted of "airy nothings," and not of actual existences in nature. The expression is faulty, because it is calculated to mislead. True poetry creates nothing. It is but a striking and beautiful image of what is already created. In other words, it is founded in truth. The book of nature is the orignal of all the genuine poetry that has ever been produced, whether by the pen, the pencil, or the chissel. The province of the poet is to give a tasteful and vivid copy on paper or canvass, or in marble or bronze, and his work is done. If the likeness be correct and spirited, the production is immortal. It is on this ground that the works of Praxitiles, Virgil, Raphael and Shakespeare will be as lasting as time. They are delineations, true to life, of what their authors admired and studied in the book of nature.

Do you ask me, in what portion of that volume its poetry is written? Well might I return the question, In what portion is it not written? It is traced in the boundless grandeur of the universe, and in the beauty, fitness, and harmony that every where prevail. Look into the heavens, by day or by night, and

you will find it in the high and bright blue arch, the radiant sun, the beamy stars, the fleecy and fantastic rack, and the thunder-cloud pouring forth its deluge and its fires. Nor will you behold it in less loveliness in the splendors of the morning and evening skies. Look on the solid earth, and you will r ad it in the depth and loneliness of the forest, in the interminable prairie decked in its fluctuating sea of blossoms, and in the lofty mountain with its drapery of clouds. Look toward the ocean, and dwell on it there, in the strife of the tempest, and the majesty of the calm. Look into the moral page, and be enraptured with it there, in the noble and generous darings of man, and the more fascinating gentleness and purity of woman. Contemplate creation, as far as mortal ken can reach, and it is its poetry, in particular, that will inspire you with mounting thoughts, and heavenly aspirations, and induce you to "look through nature up to nature's God." While the perusal of this chapter, instead of being either laborious or troublesome, will be peculiarly delightful to you, it will augment your knowledge, cultivate your taste, and improve your virtues.

But should you neglect to write, the value of the studies, to which I have referred, will be lessened by the failure. To the sick and the suffering, on whom you may attend, your attainments will be important. But that sphere must be necessarily limited. To the world, at large, and especially to posterity, your labours will be useless. Nor will they be as valuable as they might be even to those on whom they are bestowed. Unless you record it, you will be liable to forget your own experience. Singular as this may seem to you, it is notwithstanding true. Phenomena which you have previously witnessed, and cases of disease which you have treated before, will appear new and strange to you. Hence you will analyze the former with less facility, and manage the latter with less success. But journalize your observations and experience, and the case will be otherwise. The practice will serve you importantly in various ways. You will improve by it both in accuracy of thinking, and in the art of composition. The very act of recording a fact, a case, or a sentiment, will imprint it more deeply on your minds, and make

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is not the case more general? Why, indeed, is it not universal? The answer is obvious. It is found in the neglect of composition, by the great body of the physicians of our country. Abler and more successful practitioners of medicine than those of the United States do not exist. I doubt much if any others equal them. They are more independent thinkers and better reasoners, and treat diseases more boldly and philosophically, and less in obedience to precedent and authority, than the practitioners of any other country. They are much less addicted to professional routine. The same freedom of inquiry, and independence of thought, which characterize us, as a people, prevail in medicine. *Nullius addictus jurare in verba magistri*, is the banner motto of the profession; and its fidelity to it is unstained. Perhaps the most striking difference between the physicians of the United States, and those of Europe is, that, as a body, the former observe, reflect, and analyze more, but read and compose less. Hence the latter, although I think intellectually inferior, have more learning, and are better writers. But, were the American physicians to habituate themselves to composition, the case would soon be different. They would rank with the best medical writers on earth. Had I said that they would be themselves the best, I should feel no disposition to recall my words. Other things being equal, the value of composition is in proportion to the amount of intellect it contains. And, as relates to that, I am persuaded that American productions would preponderate.

Our students of medicine write too little during their pupillage. They do not consider the art of composition a part of their education. They regard it too much in the light of a mere accomplishment, with which, at their option, they are privileged to dispense. Hence their neglect of it then, and the reluctance with which they commence, at a subsequent period, a task which is burdensome to them, because it is new. Time will doubtless remedy the evil, and give to the medical literature of our country a standard corresponding to our intellect, as a people. Let me not be disappointed, then, in the hope I in-

dulge, that some progress will be made by you, toward the attainment of an end so important and desirable.

But it is not intellectual cultivation alone, however extensive and perfect it may be, that makes up the qualifications of the accomplished physician. To form such a character a confederacy of rare endowments is requisite. To the attainments of the philosopher and the scholar he must unite the virtues of the man, the sympathies of the philanthropist, and the deportment of the gentleman. His morals must be pure, his sensibilities lively, his manners courteous, and his life without reproach. And it is essential that he be possessed of magnanimity and self-control. While he feels with the keenness of woman, he should be prepared to act with the temperate resolution and firmness of man. Nothing but sensibility like this will give him the necessary interest in the sufferings of others; and such self-possession alone will enable him to meet, with his whole resources, the professional exigencies that must frequently occur to him. Nor should his delicacy be inferior to his other endowments. For all these qualities he will find ample employment, in his daily intercourse with sickness and distress. In his social capacity also, and in the offices of friendship, his conduct should be exemplary. It is a duty as binding on him to assuage sorrow, as to relieve pain; to administer the balm of consolation, as the healing balsam. His profession too is of a nature peculiarly confidential. He becomes, of necessity, an inmate of the families in which he practises. He is intrusted, therefore, not merely as a matter of course, but often, on their part, as a point of duty, with many of their secrets. This confidence, in which the feelings of females are often concerned, he should hold sacred. The violation of it would be unmanly and immoral; and, in the round of justice, the mischief done would return in an aggravated shape on himself. Treachery of this kind, dishonorable in every one, is criminal in a physician, and can never fail to cover him with disgrace, if it does not bring down on him professional ruin.

On the services of the practitioner of the healing art, requi-

sitions of charity are much more numerous and weighty, than on those of any other character. Nor should they ever be resisted by him, or listened to with indifference. On the contrary, they should be obeyed with promptitude, and the duties pertaining to them faithfully performed. The needy are most liable to disease, on account of their need, which exposes them to the influence of privation and hardship. They have, therefore, a claim, which neither benevolence nor justice will deny, on those whose lot is more happily cast. The physician, in particular, should never question it, but allow and discharge it, in its utmost latitude. His duty to this effect is dictated by every consideration that bears on the subject. I shall not assure you that your benefactions to the poor will be rewarded in Heaven. For such views I must refer you to another and more appropriate quarter. Besides, I trust the period when that recompense shall reach you, is far distant. But I do assure you that your well-bestowed charities will be recompensed on earth, by the gratification that will accompany them, and the reputation they will procure for you. They are the fruit of virtuous feeling, which, when free from selfishness, is its own reward. However contrary this sentiment may be to the dogmas of those, who, dwelling on the gloomy phantasms of their own creation, instead of looking at nature as it is, pronounce our condition on earth nothing but a scene of suffering and sorrow, it perfectly accords with the experience of philanthropy. It accords moreover with the nature of man, and the ordinances of God. In performing a deed of charity, you fulfil a law of your moral constitution. That moral gratification, therefore, should be your reward, is, from the very principles of your nature, as certain and necessary, as that fragrant odours and sweet sounds should delight your senses. Were the case otherwise, it would be a discouragement of virtue, and would argue a defect in one of the fundamental laws of creation.—These various duties of your profession have been thus presented to you, under a full belief, that, from the fair promise of your deportment, as pupils, you will, as physicians, fulfil them.

But the hour apprizes me that I have already trespassed on

your time and indulgence. I must, therefore, hasten to close my address. My feelings, on the termination of this my last act of official duty toward you, it were vain in me to attempt to express. Could I even command language suited to the purpose, I could not utter it. To your own sensibilities, therefore, I am compelled to commit the interpretation of mine. And I doubt not that they will interpret them faithfully.

The tie which has bound you to this university, in the capacity of pupils, is now dissolved, in conformity to the laws and usages of the institution. You have faithfully performed the duties required of you, and received your reward. You therefore separate from it like ripened fruit from the stem that nourished it. But something assures me that the tie which binds you to it, as friends, is still unbroken. Nor will I admit the thought that it is formed of materials liable to decay. Let me rather believe, that, like that which fastens the colours to the mast, when they wave over the heads of the determined and the brave, it will prove indissoluble, except by the summons of "dust to dust."

FAREWELL; and, in tendering to you, as the delegated organ of the occasion, the fervent benedictions of your ALMA MATER, let me add the affectionate wishes for your welfare, of those who, once your preceptors, take leave of you now, as ELDER BROTHERS.

ART. II.—*An Essay on the Absorbent System and its Functions.* By JAMES CONQUEST CROSS, M. D. of Fayette county, Kentucky.

[CONTINUED FROM PAGE 210.]

PROPERTIES of the vessels of the Absorbent System.—By a reference to Bichat's Gen. Anat. or any other work upon the same subject, the reader will readily discover, that the properties that have been ascribed to the capillary blood-vessels, to the exhalents, and to the absorbents, are nearly identical. Thus, different vascular tissues, that are radically unlike in the functions they perform, are confounded by this vague and unintelligible manner of estimating their distinctive properties. We have attempted to prove, in the 38th No. of the American Medical Recorder, and we think successfully, that the capillary blood-vessels are not in possession of a single property, in virtue of which, they are enabled either to exhale or to absorb. Between this order of vessels, therefore, and those denominated exhalents and absorbents, there can be no essential feature of resemblance. In structure, properties and function, there is, between the exhalents and absorbents, so close and striking a likeness, that it is perfectly impossible to draw between them the line of demarcation, or to point out in what respect they differ from each other. This has induced us to consider them identical, and simple fractions of the same great system of vessels.

Having in the number of the Recorder just alluded to, pointed out the nature and function of the capillary blood-vessels, we shall now take into consideration the properties of the vessels of the absorbent system, when the difference between the former and the latter will be rendered obvious.

Property 1st. The absorbents are stronger than the capillary blood-vessels.—No other evidence of the superior strength of these vessels is required, than the fact, that they are capable of

sustaining a much heavier column of quicksilver than either the arteries or the veins of the same size and caliber. Indeed, their superiority is so much greater, that though their parietes are very thin and quite transparent, they can support a column of mercury, at least, four times as heavy as that which can be supported by the blood-vessels.

Property 2nd. The fluid circulated by the absorbents is peculiar. —This circumstance has given rise to the different denominations which have been applied to the vessels by which it is circulated. Thus, Rudbeck, considering this fluid in its most conspicuous properties, very much of the nature of serosity, was induced to call them *ductus serosi;* and Bartholin, whose opinion of its nature corresponded more with that which at present prevails, denominated them *vasa lymphatica.*

It being necessary to speak in detail of the nature of lymph, in a subsequent part of this Essay, we shall defer until then, the consideration of the points in which it differs from the blood.

Property 3rd. The absorbents are alternately full and empty. This is an important, and striking peculiarity of the absorbent vessels. Upon the authority of Pechlin, Le Noble and Haller we are induced to believe, that they are sometimes empty, and sometimes distended by their contents. Even in the dead body, when engorged, they are visible, but when empty, they elude the most watchful and penetrating eye. In the same subject, they are not uniform in their appearance throughout the system. The absorbents of one part may be full of lymph, while those of another are entirely empty. It is a fact, which has been observed in animals as well as in man, that while the thoracic duct always contains lymph, the absorbents of the extremities, of the head, and of the neck, are nearly always empty. This is a happy illustration of another property of the absorbents which we shall have occasion to allude to again. While the process of chylification is going on, the lacteals are distended and may be seen, but when that process has terminated, they are empty and invisible. This is almost uniformly the state in which they are found in the dead body and arises from their posthumous action.

Property 4th. The absorbents are very irregular in their action. The want of regularity or uniformity so characteristic of the action of the absorbents, serves to distinguish them in a very eminent degree, from every other class of vessels in the animal economy. These vessels sometimes circulate their contents with astonishing velocity, and sometimes as tardily. Thus, it is not uncommon for them to be immersed for a series of years in an inundating accumulation of dropsical serum, and instead of removing it, the collection appears to be progressively on the increase, as if their energies were entirely paralyzed, and then suddenly, and in some instances without any appreciable cause, the disease is removed in the course of a very few days. By Cruikshank we are informed, that he saw chyle pervade the mesenteric absorbents with a velocity equal to twenty feet in a minute. Of six pints of warm water, says Dr. Good, that were injected into the abdomen of a living dog, not more than four ounces remained at the expiration of six hours. By professor Martini it is remarked, that he has seen, in the course of an hour, a pint of warm water absorbed from the cavity of the abdomen of both dogs and kids. In an experiment performed by Dr. Carsill, a pint of water was absorbed from the same cavity in twenty minutes. By Mr. Gohier it is stated, that water injected into the trachea of horses, through an opening below the larynx, was absorbed by the lungs almost immediately. Bile is absorbed from the cavity of the abdomen more rapidly than water: Mr. Dupuytren saw two ounces of this fluid removed almost instantaneously.

Property 5th. Retrograde action of the absorbents. Though the lymph and other fluids circulated by the absorbents usually pursue a particular route, they are not, however, constrained to obey any particular impulse or to flow in any certain direction. When attracted by an excitant or other cause of local irritation, they often deviate from their customary channels; and in particular states of the system, according to Dr. Darwin and Mr. Caldini, they make a movement perfectly retrograde.

This phenomenon is in some rare cases, and in particular portions of the circulatory circle, manifested by the blood; but

it never obtains throughout the whole sanguiferous system. It is eminently characteristic of the blood, to describe perpetually the same route, and to exhibit but few abnormal changes which do not affect its velocity.

The astonishing rapidity with which fluids taken into the stomach are transmitted to the urinary bladder, originally gave rise to the idea of the retrograde action of the absorbents. This suggestion, however, even if founded in truth, cannot with propriety, be ranked with those hypotheses, that have enlarged the sphere of modern physiology. A direct communication between the liver and kidneys was conjectured by Areteus. It was the opinion of Kratzenstein, Frankenaw, Berger and others, that the urinary bladder was endued with the faculty of attracting to it the fluids taken into the stomach, through the medium of inorganic pores, so arranged, that fluids were permitted to pass into the bladder, but not out of it. It was supposed by Bartholin, that between the stomach, intestines, and kidneys, there exists a communication through the medium of the lacteals. It was contended by Winslow, that there exists near the lumbar vertebræ, a canal which forms a direct communication between the stomach and kidneys. By Valentin and Hildanus we are informed, that they discovered foreign substances in the bladder, which they could not believe had reached it through the circulation. The case recorded by Dr. Senter, and which is quoted by Dr. Darwin, is directly in favour of a more immediate communication between the stomach and bladder, than that which takes place through the sanguineous circulation. The case to which allusion has been made, was one of ischuria, of more than three years standing. If, at any time during the whole of this period, the urine was not regularly drawn off with a catheter, it was sure to be discharged either by the stomach or the skin. There are some experiments, performed by Sir Astley Cooper, which authorize the same deduction. A ligature was placed upon the receptaculum chyli of several dogs: an effusion of chyle took place upon several of the viscera; into the cellular tissue; and upon the internal surfaces of the pancreas and *kidneys*. Dr. Darwin tells us, that

one of his friends drank of diluted alcohol until he was slightly inebriated;—in conjunction with it, he then took, nitrate of potash and asparagus. The urine in a very short time, gave out the characteristic odour of asparagus, but no evidence of its existence in the blood, could be detected. To a kid, an infusion of rhubarb was given by Rasori;—in half an hour the animal commenced urinating, and in the fluid discharged, the presence of the medicine was plainly manifested. When the animal was opened, he found the lacteals distended with a white opaque chyle, and the stomach still contained a small quantity of loppered milk impregnated with the infusion of rhubarb. The rational inference then is, that the rhubarb reached the urinary bladder through the chyliferous vessels of the intestines. The total destruction of the kidneys by suppuration, does not prevent the bladder from being filled with urine: nor when the ureters are tied, is this object attained, as was proved by the experiments of Huet and Kratzenstein.

The subjoined considerations show, that the opinion which asserts, that the absorbents do retrograde in their action, is not altogether preposterous and absurd. The fact, that these vessels are eminently endowed with vital properties, and that they, in common with the other organic tissues are liable to numerous abnormal changes, renders it not unreasonable to suppose that in the event of their being stimulated to morbid exertion, or if by any means, the valves should become paralytic, that their contents should retrograde. In the above circumstances, all obstacle to such a movement would be removed. The truth of this, is proved by the fact, that when mercury, water, or tallow are injected into the absorbents in a retrograde direction, the valves offer no resistance. It is asserted by Etmuller, Morin and Darwin, that if the urinary bladder be turned inside out, the ureters and neck of the organ being well secured and then filled with water, it will be emptied by the retrograde action of its absorbents. In several abnormal states of the system, particularly in those called diabetes and scrofula, the valves of the absorbents are diseased, and then, they present no obstacle to their retrograde action.

As we above remarked, something resembling the retrograde action of the absorbents takes place, in particular circumstances, in the sanguiferous system. By Lieutuand we are informed, that a schirrus state of the valves of the aorta will permit a part of the blood to return into the left ventricle of the heart. As dissolution approaches, these valves become paralytic and suffer the blood to regurgitate. In the veins that are supplied with valves, Baglivi says, he has seen the blood retrograde. The same phenomenon has been observed by Hunter, Marchettis, and Haller in the capillary blood vessels. It is also stated by them, that air passes without difficulty in direct opposition to the course in which the fluids are absorbed.

The insufficiency of valves in other organs, in preventing the retrograde action of their contents, is a pathological phenomenon with which we are all familiar. Thus, in the act of vomiting, the valve at the cardiac orifice does not prevent the contents of the stomach from being rejected; that at the pyloric orifice suffers the bile to enter the stomach; the valve of Bauhin, in the iliac passion, allows feculent matter to pass along the intestines in an antiperistaltic direction, and then we have stercoraceous vomiting; and De Haen injected such an enormous quantity of water into the rectum of a dog, that it was ejected from his mouth in a full stream. The bile often imparts its particular hue to the absorbent vessels of the gall-bladder, and several of the secretions are occasionally impregnated with it, from its re-absorption. The puncta lachrymalia, together with the lachrymal sac and nasal duct, appear to form a gland, that has some resemblance to the intestinal tube. Thus, when the nasal duct is obstructed, it takes on a retrograde action, and the tears are again poured into the lacus lachrymalis.

These considerations firmly establish the fact, that valves are altogether incapable of presenting any serious obstacle to the retrograde motion of the contents of the organs in which they are found. Indeed, if there is a tendency in the absorbent vessels to this kind of action, which the valves are constructed to prevent, they should exist in all animals in which these vessels have been found. This, however, is not the result of the re-

searches of the comparative anatomist. Thus we are told by Mr. Hewson, that although absorbents abound in fish, these vessels are not furnished with valves. Nor are they uniformly to be found in the economy of man, as in a multitude of white pellucid vessels, evidently absorbent in character, they could not be detected by Nuck, Ruysch, Vieussens, Helvetius and Senac. Moreover, if valves were designed to answer no other end, than to prevent the retrograde action of the vessels in which they are found, their number and their manner of distribution, should have been regulated exclusively upon the principle of necessity. This rule, however, is never observed; for instead of acquiring strength and augmenting in number, in a ratio corresponding with the size of the vessel, they decrease in number as the vessel increases in magnitude. Thus, in the thoracic duct there are but few, while they abound in those vessels of a much smaller caliber.

Property 6th. When abnormal, the absorbents take up fluids indiscriminately. In consequence of the great variety of morbid changes which the sensibility of these vessels sustains, they are enabled to absorb every species of fluid, produced by or artificially introduced into the animal economy. This, however, never takes place to any extent but in disease, and then, its occurrence is so frequent, that illustrations of it may be derived from every morbid affection to which the human body is heir. Thus they have been coloured blue by indigo; yellow by the yolk of the egg and the yellow-beet; red by the beet of the same colour. Hydropic serum has been found in them in great abundance, as well as bile, urine, milk and pus. In these vessels, we shall find the theatre of hæmorrhage, as well as the throne of inflammation.

Property 7th. The absorbent vessels are endowed with the faculty of re-production. As we have already remarked, these vessels possess the power of vegetating, elongating, reproducing and of ramifying into surrounding parts. The existence of this faculty is deduced from the manner in which parts grow and unite when they have, from violence sustained a solution of continuity; from the disappearance of solid tumours during life, together with the ves-

sels by which they were supported; by the experiments of Brugmans, by which it was demonstrated that cicatrized surfaces recover the faculty of absorption, which could only result from the growth and regeneration of the absorbent vessels. This is a property which belongs neither to the arteries nor the veins. These vessels never pullulate into the tumours with with which they are frequently surrounded, nor are they ever found pervading cicatrices. The chief pathological phenomena manifested by them consist in an augmentation or a dimunition of their caliber. The morbid characteristics of fungus excrescences have no tendency to invalidate the truth of this assertion. The congestion with blood, of the vessels of such tumours, furnishes no evidence that they are either arteries or veins. When these tumours have existed a considerable length of time; have become more aggravated; and are on the point of becoming carcinomatous; these same vessels, instead of being engorged with blood are distended with substances albuminous and adipocerous in character.

Property 8th. The absorbents are more tenacious of the vital principle than any other tissue. No one, I believe, doubts that the absorbents continue to act some time after dissolution, although, there is considerable discrepancy of opinion, as to the particular time when their functions altogether cease. Thus it is remarked by Haller, "Quietis tamen intestinis, et multa morte frigidis, chylum moveri et effulgere." By Bichat we are told, that "organic sensibility, and insensible organic contractility continue some time after death. A fluid injected either upon the serous or mucous surfaces, while the animal is still warm, is absorbed." He asserts that the subsidence of heat, and the cessation of absorption, are simultaneous. Mr. Shaw says, that absorption continues after the animal is deprived of sensibility, for if the thoracic duct be then tied, the lacteals will become distended. They possess, according to Dr. Horner, striking powers of spontaneous contraction in the dead body, though he attempts not to determine when this power ceases. It is said by M. Magendie, that absorption continues to go on for a considerable time after death. He remarks, that "after having emptied

one or more of the lacteals in an animal recently dead, by compression, we shall again see them, in a short time filled up anew." This experiment was performed after the animal had been dead two hours. It is asserted by Mr. Abernethy, that when individuals die with œdematous limbs, the œdema disappears. It is a common post mortem observation, to find, that while the thoracic duct contains more or less lymph, the absorbents of the extremities, head, neck, &c. are entirely empty. Mascagni, who is doubtless a most talented and experienced judge, asserts confidently, that absorption continues after the animal becomes cold. This is the opinion of M. Desgenettes, who likewise remarks, that the younger animals are, the powers of absorption are the more active, and continue the longest after death. It is affirmed by Soemmering, that the powers of absorption survive those that produce animal heat: for a case occurred to him, in which after the body had become cold, a blackish fluid contained in the bowels was removed by the intestinal absorbents.

This property we have, perhaps, treated with more prolixity, than the reader may be inclined to think it merits: we are persuaded, however, that when he becomes acquainted with the extensive and important application we shall make of it, he will not be disposed to censure us for bestowing upon it an undue portion of attention.

Property 9th. The absorbents possess Sensibility. Clinical observation daily evinces the existence of this property. It is manifested in the readiness with which they inflame when noxious matter has been absorbed, and by the pain which is then experienced. Their frequent inflammation from disturbance of the general constitution is also a proof of it. A common cold produces a painful tumefaction of the absorbent glands; and in some varieties of fever these parts are particularly obnoxious to disease. This was especially the case with a fever that prevailed at Naples, and of which Dr. Sarcone has given us a faithful history. A case of fever recently fell under my notice, in which the whole sub-cutaneous cellular tissue appeared to be in a state of suppuration.

Property 10*th*. *The absorbents are contractile and irritable.* By Haller we are informed, that the vessels of the absorbent system are contractile and possess a degree of irritability superior either to the arteries or veins. Their muscularity and irritability are facts which were considered by Cruikshank incontestible. That two of their tunics are muscular, is not at all improbable, as muscular fibres have been detected, not only in the thoracic duct of the more unwieldy animals, but also in that of man. In the former they were observed by Schelden, and in the latter by Schneider. Under the action of chemical irritants, says Dr. Cruikshank, they discovered irritability. The thoracic duct has a living contractile power, say M M. Gmelin and Tiedemann, for when it is tied and punctured the chyle springs out like blood from an open vein. This does not depend, as Mascagni and Bichat suppose, upon the exertions of the elastic parietes of the duct, but is the result of a living contractile effort. Of this we are assured by the fact, that if the duct be tied and punctured a certain time after death, the chyle instead of springing from the duct like blood from a vein, exudes slowly from the orifice. In a recently dead animal, if the chyle be discharged by puncture, the duct immediately contracts. This phenomenon is manifested by all the absorbents, and is illustrated by the absorption of chyle; for after that process has terminated, they become invisible. If the abdomen of a lamb be opened three hours after having drank freely of milk, the mesenteric absorbents will be found filled with a whitish fluid, which they manifestly circulate by a contractile effort. They not only empty themselves with great promptitude when exposed to the action of cold air, but contract throughout their whole length, precisely as the muscles do when subjected to the action of an irritant. Professor Lauth, of Strasbourg, killed a dog about the time the process of digestion was terminating, opened the abdomen immediately, and found the lacteals filled with chyle; but these vessels irritated by the contact of air immediately contracted, and in the course of a few minutes they were invisible. The result is the same, says the Professor, if the abdomen be opened at any time within twenty-four hours; but at the

expiration of this period, their irritability is exhausted, and then, though we permit the air to act upon the chyliferous vessels, they continue distended with chyle.

The muscular contraction of the absorbents, we are informed by Dr. Alexander Monro, may be distinctly seen in the sea egg, or star fish, in which it is very perfect: in these animals, there are no valves to assist in determining the contents of these vessels toward the heart.

The contractility of the absorbents is beautifully displayed in the removal of the tears from the eyes. This is doubtless accomplished through the agency of a process purely vital. Each of the lachrymal ducts, by a peculiar and specific action, absorbs the tears accumulated in the lacus lachrymalis and determines their flow into the sac. If the absorption of the tears is not under the control of vitality exclusively, why is it that, in many fatal diseases, as the patient gradually sinks, the tears are not as rapidly removed as in perfect health? If mechanics have any participation in this process, no impairment in its energy should be observed, for the physical integrity of the parts continues unaffected. But an observation of M. Richerand removes all doubt on this subject. We are assured by him, that he saw the puncta lachrymalia contract in a child when the instrument employed by him did not immediately enter the canal, and he found it necessary to wait some time for this contraction to subside, before he could succeed in his operation.

In the turtle, the absorbents continue to act after decapitation; for they absorb after the removal of the brain. Contraction is essential to the performance of that function, and here we are presented with a striking analogy between the absorbents and the muscles; for in this amphibious animal the latter contract several hours after the loss of the encephalon.

With regard to the lymphatic glands, which are supposed by many to consist of nothing more than a congeries of absorbent vessels, it was contended by Santorini, that they are in possession of a peristaltic motion. By Lancisi it is asserted, that they are muscular, and that they contract and dilate like the heart. This opinion was adopted by Mylius. By A. F. Hoffman sphincters

are ascribed to the glands; in virtue of which, fluids are admitted into them or rejected in correspondence with the peculiarities of their nature.

There is a peculiarity in the manner in which the absorbents sometimes discover their irritability. When one of these vessels is pricked by a pointed instrument, horripilation or a chilly sensation, not unlike that characteristic of the first stage of a paroxysm of intermittent fever, is produced. This singular phenomenon has been observed by Hewson, Hunter and Cruikshank; by whom it is also remarked, that it is accompanied by a general malaise and by red painful lines which correspond with the course of the vessel. The absorption of a virus will produce the same effects.

Property 11*th*. *The absorbents possess elective sensibility.*—It has been already shown, that the different vascular tissues of the animal economy, although they circulate fluids radically different in their essential character, have actually a direct communication with each other. Though this is the case, their respective fluids need not necessarily and in a confused manner mingle together. In health this never happens: but in disease, it invariably ensues to a greater or less extent. There is a power, which presides over the action of these tissues, by which in health, the fluids of the animal economy are confined in their proper vessels; which prevents foreign or unassimilated substances from admixture with them; and that readily admit those with which it is in relation.

This power is denominated by Bichat, organic sensibility; by Burgmans, vita propria, and by M. Alard elective sensibility. The latter seems to express most fully the nature of this faculty; by us, therefore, it shall be adopted.

The existence, however, of this elective sensibility, is not one of those incontestible points, that admits of no difference of opinion among physiologists. On no subject, indeed, connected with the science of life, can opinion be more opposite and contradictory. While some very enlightened individuals, place the most unqualified faith in the existence of this property, others whose sentiments are perhaps as much entitled to respect, con-

sider it a fable, as wild and extravagant, as the dreams of the latter Platonists. The evidence however, on this debated point, decidedly preponderates in favour of the sentiments maintained by the former.

The language held on this subject, by a recent writer, the novelty of whose opinions has made him extensively known to the profession, is in a strain of illiberal denunciation, discreditable to himself, and prejudicial to the interests of science. By him it is said, "that the notions of a peculiar unintelligible vital power of discernment and appropriation, existing at the ends of the absorbing radicules, *cannot even be alluded to*." It is infinitely more easy to denounce, than to disprove; to use the language of the dogmatist, than the language of reason and intelligence. The motive, however, of this sweeping verdict of condemnation is apparent. Before the bar of reason, it is impossible for the opinions of Dr. David Barry, and that which contends for the existence of the property under consideration, to be acquitted. They wage against each other a war of mutual extermination. Science will not recognize them both as her legitimate offspring. The bastardy of one is the necessary result of the adoption of the other.

It cannot be doubted, that so long as the laws which are peculiarly vital, are held in higher estimation by the physiologist, in the elucidation of the phenomena of life, than those of a physical character, the opinions of Dr. Barry cannot constitute any portion of fixed and unfluctuating science. The essence of the principle of vitality is necessarily inscrutable. To attempt to comprehend it would be visionary and frantic, and a daring invasion of the province of the Deity. To understand its laws, should be the extent of our ambition: it is all that we shall ever be able to accomplish. In reference, therefore, to the property under consideration, it is enough for us to know, that it actually exists; that it is a modification or peculiar state of vitality; that it is one of the general laws of life; that it is itself amenable to certain fixed laws; and that its effects can be appreciated.

The existence of the property denominated elective sensibility, rests upon sufficient evidence to entitle it to be ranked

among the other laws of life. The admission of its existence, furnishes us with a satisfactory and intelligible solution, of numerous phenomena both normal and abnormal, which, without it, we are totally incapable of understanding. Indeed, it is a property, which even the ultra-mechanical physiologist would be constrained to admit, if he would suffer himself to contemplate with a mind unembarrassed and uninfluenced by the prejudices and predilections of hypothesis, the operations of the economy as they are naturally carried on, and not as they are distortedly exhibited in his experiments.

None of the mechanical physiologists, can assign a reason, upon the principle of capillary attraction, or of atmospheric pressure, why chyme is absorbed by the lymphatic-absorbo-exhalents of the intestines, and the feculent matter and bile rejected. To grant to these vessels a selecting faculty, which accurately corresponds with the phenomena exhibited in the bowels, every difficulty is removed. If it be true, of which there is strong reason to doubt, that oxygen is the only atmospheric element absorbed by the pulmonary venous absorbo-exhalents, upon what other principle can the rejection of the azote and carbonic acid gas, which are with it chemically combined, be explained? In the process of sanguification, pulmonary serosity is excreted by the arterial absorbo-exhalents of the lungs, but why do not these vessels absorb the blood? Their elective sensibility presents to it an insuperable barrier. The liver, as we shall attempt to prove, is nothing more than a depurating organ; and if so, why does the blood of the vena portarum, give up to the action of the pori biliarii its impurities only? For no other reason than that which has been just assigned. Usually, the bile in the gall-bladder is not absorbed, but why is it in jaundice acted upon by the venous absorbo-exhalents and carried into the venous circulation? The elective sensibility is here equally operative. In the proper place, it is our design to attempt to prove, that lymph is nothing more than chyle which has not been sufficiently assimilated, and which, for re-elaboration is removed from the capillary arteries by the lymphatic-absorbo-exhalents. If so, why is the blood rejected?

It is unnecessary to say, that we are unable to assign any other good reason, than the agency of the elective sensibility, which presides over the action of their absorbing radicules. A thousand other illustrations might be adduced, but these are abundantly sufficient to prove the existence of this faculty: nor should we deem it right to intrude upon the attention of the reader additional evidence, if it were not the result of actual experiment.

Thus, a ligature was thrown around the axillary artery of a living calf by Professor Buniva; the vessel was punctured below the ligature; and then blood diluted with water was injected into it. Although considerable force was applied to the piston, such resistance was made, as to make the entrance of the fluid a matter of difficulty. In some experiments previously performed upon the dead human body, the injection soon made its appearance upon the skin, but in this, nothing of the kind occurred. When the dead body was injected, the skin assumed the colour of the blood: the palms of the hands put on a beautiful rose tint; blistered surfaces were swelled, reddened, and covered with drops of diluted blood; the liver, spleen, stomach, intestines, all the membranes, most of the humours of the eye, the periosteum and the bones, exhibited the appearance of violent inflammation. After it was ascertained, that the fluid injected would not appear upon the surface of the living animal, the spinal marrow was divided near the occiput. The heart almost immediately ceased to beat, and the animal expired in a few moments. Life however had hardly become extinct, when the fluid that still remained in the syringe, entered the artery readily; without resistance; made its appearance upon the surface of the body; and upon most of the abdominal viscera. Why was the entrance of this fluid resisted in the former, and suffered in the latter? This difference could only arise from the barrier presented by the elective sensibility in the former, which was removed in the latter. In the former, the arteries are always a plenum; it is therefore, very clear, that unless the foreign fluid could obtain admission into some other vascular tissue, the resistance alluded to, must uniformly be made. This was prevented by the elective sensibili-

ty, which presides over the absorbing radicules of the arterial absorbo-exhalents, the only order of vessels that originate from the capillary arteries, and terminate upon the surface upon which the injection made its appearance. After the animal had expired, this barrier was removed; and we have already seen with what facility and promptitude the injected fluid pervaded these vessels.

The very recent experiments of M. Martigney, are perhaps no less conclusive than those of Professor Buniva. Into the stomach of animals killed the previous day, M. Martigney injected a weak solution of hydrocyanate of potass, and four minutes afterwards he observed, that the outer surface of the stomach became of a uniform blue colour when tested with the sulphate of iron. He then repeated the same experiment on living animals with the following results. Twenty minutes after the injection of the hydrocyanate, when none of it had reached the outer surface of the stomach, he poured several drops of the sulphate of iron over it. It was not till five minutes afterwards, that he remarked here and there a faint blue tint not uniformly diffused as in the former instance, but arranged in a fine network; gradually the tint deepened and extended itself, its reticulated structure became less distinct, and at length a uniform blue stain was formed as in the dead animal. It is a point, which I feel persuaded cannot be disputed, that it is quite impossible to explain satisfactorily the rapid introduction of the fluid into the vessels of the stomachs of the dead animals, and its slow progress in those of the living, unless we admit them in the latter case under the control of vitality and not in the former; unless we attribute the rapid introduction of the fluid in the former to the absence, and its tardiness in the latter to the presence and direct agency of the elective sensibility.

The specific nature of the action in which the property of the absorbent vessels under consideration consists, is of necessity molecular; and therefore, unavoidably, invisible and undemonstrable. Its existence, however, is recognized at the extreme points of the absorbent radicules, as is evinced by numerous phenomena, several of which have been already placed

before the reader. The discovery of a fluid in these vessels, immediately after having passed their roots, essentially unlike that, in which they are frequently immersed, a fact which has been repeatedly testified to, is abundantly sufficient to justify the assertion just made with regard to the seat of the property denominated elective sensibility.

Our inability to reveal the nature of this property, should not be considered as having any tendency to invalidate the arguments adduced in its support; for it is upon an equal footing with numerous other organic actions, especially what have been called secretions, the nature of which is just as inscrutable. The numerous and diversified results of elective sensibility, are familiar to the mind of the enquiring physiologist, and this is all that is, or can be known of the organic actions in which the functions of secretion consist. Their existence, therefore, rests upon evidence precisely identical in character; and yet, there are those who unhesitatingly assail the former, while they suffer the latter to repose in unmolested security.

The presumption, however, if speculation be allowable on so abstruse a point, that elective sensibility consists in alternate contraction and dilatation, is not altogether unfounded. Thus we suppose that the muscular fibres of the radicules of the absorbents, under the presiding influence of elective sensibility, dilate them, by which they are accommodated or adapted to those substances of a mild and unirritating nature, while by a contractile effort, those of an opposite character are rejected. In illustration of this, we are informed by Mr. Monro, that the colouring principle in wine, is much more minute, than the globules of milk; notwithstanding which, the former is rejected, while the latter is absorbed by the lacteals. It appears that the puncta lachrymalia in the performance of their functions, go through evolutions, bearing a close resemblance to those which we ascribe to the absorbent radicules. Indeed, I imagine the only difference between the former and latter, consists in the one being on so large a scale, as to be cognizable to the senses, while the other is so minute, as to be invisible. In one respect, at least, they correspond precisely, for the essential nature of

the one is as inscrutable as that of the other. The whole amount of unfluctuating knowledge acquired with regard to either of them is, that they are active and not passive agents in performance of their functions; and this, constitutes an irresistible argument against all those hypothetical explanations of the process of absorption, which have been deduced from the principles of the physical sciences.

Property 12th. There is an action belonging to the vessels of the absorbent system denominated elaborative. By this phrase, we understand the changes which ensue in the nature of the contents of the absorbent vessels subsequently to their absorption. By this process, the state of the substances introduced into the economy is progressively improved, until, at last, they are rendered fit to enter into the structure of the living solid.

Although there may appear a likeness so striking, between the elaborative action of the absorbents and their elective sensibility, as almost to authorize the consideration of them as identical, there is in every respect a manifest difference between the former and the latter. Thus, the functions ascribed to these properties are separate, distinct and characteristic. While to the elective sensibility, is assigned the office exclusively of selecting suitable materials; upon elaboration devolves the duty of moulding them into an appropriate form, and finally, to exhalation belongs the process of depositing them in the parenchymatous tissues of the body.

The existence of the property under examination, is inferred from the following considerations. The absorbent vessels are very minute, and it is, in vessels of capillary magnitude, that the changes which take place in the substances introduced into the economy are produced. The vessels of this system, seem to be too minute, to have no other office than that of mere conduits to perform. The circulation of the contents of the absorbent vessels is very slow; the design of which is doubtless to allow the vessels sufficient time, to produce upon them the necessary changes. The chyle, before it is mingled with the blood, is incorporated with the lymph; and finally, in its passage to the blood, the chyle goes through the mesenteric ganglions;

and these organs, are generally regarded as agents of elaboration.

The evidence, however, above adduced in support of the elaborative action of the absorbents, is altogether of a presumptive character, and the institution of an examination of the contents of the duodenum, thoracic duct and its tributary branches, is necessary to be furnished with data, from which the truth can be deduced.

Many years ago, from a number of experiments instituted by Werner, he inferred the acidity of chyme, while he found the chyle to be destitute of that attribute. The very recent researches of MM. Tiedmann and Gmelin are of a peculiarly conclusive character: they have rendered the non-identity of chyme and chyle a point no longer contestable. Without entering into detail, it is sufficient to say, that they have satisfactorily demonstrated, that perfect chyle is to be found only in the thoracic duct, and that chyme is a fluid of very dissimilar characteristic attributes.

The difference between chyme and chyle is not more remarkable, than that observed in the fluid taken from different parts of the elaborative apparatus of chyle. Thus it is asserted by Ruysch and Cowper, that as the chyle issues from the mesenteric ganglions, it appears more clear, limpid and aqueous than when it entered them. It is said in general terms by Professor Schultz, of the University of Berlin, and it appears that he speaks from actual observation, that the chyle, after it enters the lacteals, is progressively elaborated, especially in the lymphatic ganglions, which he compares to the articulations of vegetables; particularly the *Charæ*.

The chyle, we are told by MM. Emmert and Vauquelin, although as white as milk in the lacteals, assumes gradually, especially in particular points, a faint reddish hue after it has entered the thoracic duct, and that this colour deepens as it approaches the subclavian vein. By MM. Halle and Nysten, it is affirmed, that the chyle in the thoracic duct never fails to redden when exposed to the action of atmospheric air. By MM. Reuss, Tiedmann and Gmelin, it is stated, that the chyle, before

it enters the ganglions, is of a yellowish white; that is is not reddened by exposure to the action of atmospheric air, but that after it has passed into the thoracic duct it reddens readily.

On the subject of its coagulation, MM. Tiedmann and Gmelin remark, that while chyle will hardly coagulate at all before, it will most readily after having passed through the mesenteric glands. MM. Emmert, Reuss and Vauquelin, agree in representing the chyle in the *sublumbar* branches, as more imperfect and ill-defined than that of the thoracic duct. That examined by MM. Emmert and Reuss, when compared with chyle taken from the thoracic duct, was found to coagulate much more imperfectly.

The fibrin, says M. Vauquelin, which enters into the composition of chyle, becomes progressively more abundant as it approaches the thoracic duct. We are informed by MM. Tiedmann and Gmelin, that after the chyle has passed through the mesenteric glands, the fibrin begins to appear, and that it is much more abundant after the addition of the lymph from the spleen which contains a very considerable amount of it. By the celebrated Dr. Prout it is stated, that the proportion of albuminous matter, and especially fibrin, appears to be much less, or at least, these principles exist in a much less perfect state in the chyle as immediately taken from the intestines, than in this fluid as it exists in the thoracic duct and about to enter the sanguiferous system.

Thus it is established by evidence altogether unimpeachable, that the chyme from the moment of its removal from the duodenum, until it is exhaled into the circulation, undergoes a series of successive changes, which progressively assimilates it more and more to the homogeneous nature of blood.

With regard to the absorptions, which take place in the capillary arteries by the arterial absorbo-exhalents, the same vital elaborative action may be satisfactorily demonstrated. Without, at this time, closely studying the nature of the secernent functions, we are authorized to assert, as an admission almost universally made, that the blood must assume a new form before it can be considered a secreted fluid. That the asumption of this new

form is not a phenomenon attributable to the action of the blood-vessels, is the only point necessary for us to attempt to establish at this time. The blood must, therefore, be traced as far as possible into the minute ramifications of the arteries, in order to ascertain if any preparatory change can be detected in it, while it remains in the torrent of the circulation. Let us suppose that it has reached the most minute capillary blood-vessels of a secretory organ: do we there find, that it has sustained any prelusive elaborative change? Do we there find it in any respect, different from the blood of any other part? No, unquestionably. We observe it the same identically, at the point at which its supposed secretion takes place, that it was, when forcibly propelled from the left ventricle of the heart.

In defiance, however, of the most direct and positive evidence, Professor Dumas earnestly contends, that a blood in possession of peculiar attributes, is carried to each secretory organ. This, however, is an ingenious though inefficient attempt to revive a very antiquated and exceedingly absurd hypothesis. Long before his genius added lustre to the school of Montpellier, the opinion of M. Dumas, was familiar to his professional ancestors. In the mouldy records of ancient physiology, we find it suggested, that the blood distributed to the superior organs of the body is more amply provided with oxygen and caloric, in order to prepare it for the elaboration of the more ærial and gaseous fluids, which it was supposed were secreted by them, while that, which was sent to the inferior parts was more abundantly furnished with carbon and oil to form the grosser secretions. These fancies deserve to be considered as the deductions of sound philosophy, when compared to the vagaries of the imagination of Nesbit. This physiologist contended, that the secretory organs exercise to a considerable distance a preparatory influence upon the blood, to enable it to be converted with more readiness into a new form after it had entered them. To expose the absurdity of these visions of fancy, we have only to compare the arterial blood of one organ with that of another. No physical or chemical difference can be detected throughout the whole

arterial system: this blood is perfectly uniform and identical in character.

Since, therefore, chemistry with its multifarious means of detection, has not been able to discover the slightest shade of difference, in the blood contained in one part of the arterial system, from that in any other; we are surely warranted in condemning, as false and unfounded, any hypothesis which supposes, that a preparatory change is produced upon the blood in the capillary arteries. The elaborative change which is manifested in all the secretions, must consequently, be accomplished in vessels that set out from the capillary arteries, but which are essentially different from them. In numerous abnormal conditions of these vessels, elaboration cannot take place; and then they are engorged with blood in its formal state; which is illustrated by the phenomena of inflammation.

A denial of the existence of this property, would at once lead to conclusions, which every enlightened physiologist must, upon a moment's deliberate reflection, condemn and disavow. Uninfluenced by its agency, the food must pass through the stomach unaltered; the chyle must consist of undigested alimentary substances; and if it were possible to make blood, the secretions would of necessity consist of that fluid in the state in which it is found in the arteries.

Property 13th. An exhalatory action is a conspicuous attribute of the vessels of the absorbent system.—Prolegemenous to the particular consideration of this property of the absorbent vessels, the reason why we prefer the phrase *exhalation* to that of secretion, which we shall uniformly use throughout the remaining pages of this essay, should be made known to the reader. The vessels of the absorbent system, seem to perform three offices, that are separate and distinct: *i. e.* absorption, the process by which a substance is originally taken up; elaboration, that by which its primitive character is changed; and exhalation, that by which the result of elaboration is poured, deposited or distilled into or upon a part. To neither of these processes, is the word secretion in any respect applicable. Secretion we understand, means to separate; from which the reader would

infer, that the vessels denominated secernents, separate from the blood a part of one or more of its elements. To this it may be objected, that it has not been satisfactorily proved, that any such separation of the constituent principles of the blood takes place: on the contrary, it is the author's design, to attempt to prove, that in ministering to many of the exhalations, particularly the nutritive, the blood is absorbed *en masse*. In some instances, it is impossible for any such separation to take place; for it is known, that the chemical elements of some of the exhalations do not exist in the blood: therefore, to suppose their separation from it, is absurd.

If it were true, however, that a separation of the elementary principles of the blood does take place, the word secretion, is evidently too limited in its signification. It makes no allusion, to the supposed mode by which the element thus separated is introduced into the *secernent vessels* as they are denominated. If the exhalations are removed from the blood of the capillary arteries, by a vascular tissue distinct from the blood-vessels, and are not deposited in the parenchymatous textures or their appropriate reservoirs by the capillary arteries themselves, it is a fact, the truth of which cannot be questioned, that it must be through the agency of a process of real absorption. In some of the peculiarities of absorption, a separation does take place in the substances exposed to the action of the absorbent vessels; but this is accomplished through the agency of the presiding energies of elective sensibility. The process denominated secretion, analogous, therefore to absorption, viewed in all its peculiarities, will be much better understood by being ranked with it, than by the retention of a word, which in the utmost extent of its signification can do nothing more than imperfectly express a part of the process it was intended to designate.

In reference to the second office, *i. e. elaboration*, ascribed to the absorbent vessels, the word secretion is altogether destitute of significancy: indeed, it never was designed to express any such property. Here no separation takes place—the whole contents of the vessel are to be acted upon—they are to pass through a series of successive assimilative changes, until they

are sufficiently animalized to enter into the intimate structure of the living solid.

When we compare the word secretion with the phenomena of the third and last office, *i. e. exhalation*, ascribed to the absorbent vessels, we discover it to be equally inexpressive. The whole of the proceeds of elaboration are found to be distilled in the form of gastric juice or saliva, fibrin, or phosphate of lime, and not an integral part as the phrase secretion clearly signifies.

In proposing this innovation, we are aware, that we introduce no new term, we only extend the use of a word already familiar with the profession. The term to which we have given an enlarged and more comprehensive signification, has been employed by authors to designate the separation of noxious articles from the blood, while, that which we have rejected has been used to express the separation of those that answer some useful purpose. This distinction is purely hypothetical and without foundation. The exhalations, whether excrementitial or recrementitial, are destined to answer equally useful purposes. Their regular production, are alike indissolubly connected with the integrity of the functions of the animal economy. The exhalation of the lungs is excrementitial, while those of the nutritive organs are recrementitial; but the suspension of which will produce the most immediate destruction of life. Bile we have ranked with the former, while gastric juice is associated with the latter; and yet, we know, that from abnormal states of the former the most dangerous diseases frequently arise, while those from the latter are comparatively rare.

With the above explanation, which we would have had to enter into at a subsequent part of this essay, had we not have given it at this time, the reader is qualified to understand us when we say, that "*An exhalatory action is a conspicuous attribute of the vessels of the absorbent system*," in proof of which the evidence shall be adduced.

The existence of the arterial absorbo-exhalents, as we have denominated them, cannot we are persuaded upon rational grounds be questioned. That they are an order of vessels essentially different from the capillaries in which blood is circula-

ted, is a point, which we esteem equally incontestible. That they are organs of exhalation, is no longer debateable, and that in removing the different elements of, or the blood itself from the arteries, by the exercise of a process purely of absorption, is self-evident. To all but those under the dominion of an interested scepticism, these considerations are sufficient to prove, that the absorbents possess the property above ascribed to them. To render, this, however, as manifest as possible, we deem it necessary to enter into such details, as we think should remove every doubt from the minds of the most obdurately incredulous.

To ascertain how confused and unintelligible are the views of the ablest anatomists in regard to the minute vascular tissues of the body, we need only have recourse to the General Anatomy of Bichat. By assigning a series of properties, that are almost identical, to the capillary blood-vessels, the absorbents and vessels which have been called secernents and exhalents, that illustrious individual has produced a chaotic confusion absolutely unintelligible. An investigation of these subjects soon convinced me, that he had committed a most egregious error in the ascription of properties to the capillary blood-vessels to which they are by no means entitled. An exposition of this error was made by the author on a former occasion. The striking resemblance between the properties of the absorbents and exhalents as they are described by Bichat, which upon enquiry I felt persuaded was correct, originally suggested to me the idea, that the exhalents were nothing more than a distinct integral part of the absorbent system. It is impossible to believe, that properties, that are identical, can proceed from tissues, that are of dissimilar organization..

The researches of the anatomist prove, that there are but few tissues so amply supplied with absorbents, as those in which the exhalations are most abundant. The liver and kidneys are examples, which prove this to be true. Although they are the exclusive agents of solidification and fluidification, they are more numerous than the accomplishment of these purposes seem to imply. It should be remarked also, that in all the exhalatory

organs the cellular tissue is quite abundant; particularly in the mammæ and in the salivary glands. In the more compact organs it is true, that this tissue is not always very manifest, but this is ascribable altogether to their colouring matter: for maceration, and the facts of pathological anatomy clearly evince its existence.

In reference to the particular properties so characteristic of the absorbent vessels, examination will convince the reader, that their existence as attributes of the exhalents can be satisfactorily proved. Thus contractility is manifested in the latter in the distillation of the products of elaboration: in the performance of this particular office, this property is indispensable.

When inflamed the exhalents are acutely sensible; and it is only under such circumstances, that the absorbents are painful.

That they have a retrograde action is proved by Dr. Monro, who says, that he discovered the excretory canals of the lachrymal glands by plunging the eye into a coloured fluid, which was absorbed by them. This was unquestionably in a retrograde direction.

When abnormal, the exhalents are pervaded by fluids of every kind. Thus it is remarked by Zacutus Lusitanus, that the fever with which a patient of his was annually attacked, always terminated spontaneously by an abundant sanguineous perspiration. By Dr. Merat we are told, that his clinical observation has furnished him with instances of the exhalation of blood upon the skin, the axillæ, the legs, the hands, the skin the cranium, &c.

The exhalents like the absorbents are alternately empty and in a state of plenitude. In the language of Bordeau they all sleep or wake when irritation affects them, or determines their immediate or sympathetic action. Thus the saliva is exhaled more abundantly during mastication; gastric juice during digestion, &c. When these processes have terminated these exhalations are nearly suspended.

The action of the exhalents is quite as irregular as that of the absorbents. They are diminished or suspended and augmented

or exuberantly abundant. Illustrations are familiar to the youngest physiologist.

The exhalents are as tenacious of life as the absorbents. Thus Professor Sparenza speaks of a young woman who died of encephalitis on the fourth day from the seizure. Twelve hours after dissolution, the corpse was covered with an abundant sweat, and the surface was still warm. At the expiration of twenty-four hours, the body was cold, but the face and neck were still covered with moisture although the skin had been frequently wiped dry. Several similar cases were found by Sparenza on record. Animal heat, we feel persuaded we shall succeed in proving, is a product of exhalation; and if so, we may adduce the case of Morgagni, in which the generation of heat continued during the space of three days after death, though in the month of January, in proof of the posthumous action of the exhalents. It is remarked by M. Ruillier, that the nails and hair continue to grow after dissolution; that vital heat is generated, and that several of the other exhalations, particularly that of the kidneys survive the extinction of life in the lungs and heart.

That the exhalents like the absorbents are reproduced is satisfactorily proved by the reunion of divided parts, and the regeneration of those that have been lost.

Elective sensibility is quite as eminently displayed in the exhalents as in the absorbents. This species of sensibility accommodates the exhalents to the particular nature of the materials, that are qualified to form each exhalation. It is by this sensitive tact that they attract, distinguish and absorb fluids of a suitable character.

The elaborative action of the absorbents is not more remarkable than that of the exhalents. Here we are forcibly struck with a most remarkable analogy between the former and the latter; for while it is an attribute of the former, to impart a new form to whatever is submitted to their action, the products of the latter are, to an equal extent, characterized by the legible marks of vitality. Thus, how various are the peculiarities of the saliva—how easily is its nature changed by the impressions that are made upon the animal? Intense pain, even remotely situa-

ted, or violent mental excitement, may in a short time render that which was bland and inodorous, an acrid, irritating, corrosive, fœtid and tenacious fluid. We often see fluids discharged from the stomach, intestines, liver, bladder &c., that resemble any thing else, as much as they do the natural exhalations of those organs. How are we to account for these morbid products, if we do not attribute them to the abnormal elaborative energies of the exhalents?

In addition to the above analogies, it may be remarked, that absorption is more vigorous in childhood than at subsequent periods of life; and according to Dr. Davy, animal heat continues habitually higher in the infant than in the adult. Absorption is protracted after dissolution, for a greater length of time in those who die during youth than in advanced years, and vital heat is generated longer in those who die young than in those that expire in the meridian or decline of life. In those that die suddenly, absorption continues longer than in those who fall victims to lingering diseases; and animal heat while it continues to be generated in the former for hours and even days, according to Morgagni, in the latter, the power of producing heat is at once destroyed, and an icy coldness never fails to seize upon the corpse almost immediately.

In possession of the preceding full and ample account of the numerous analogies of the absorbents and exhalents, the reader, we feel persuaded, will unite with us in considering the order of vessels which we have denominated arterial absorbo-exhalents, as constituting an integral part of the absorbent system. And, if so, the exhalatory action which we ascribed as a property to the vessels of this system is incontestably established.

We here draw to a close our account of the properties of the vessels of the absorbent system. We have been more elaborate than we could have wished; but this was unavoidable: the nature of the subject would not allow us to consult brevity. In all our future disquisitions, either of a physiological or pathological character, the necessity of a thorough acquaintance with these properties will be apparent. To avoid repetition in future,

the author determined to treat the subject as fully and as satisfactorily as possible on the present occasion.

Mechanism of Absorption. The physical sciences have been laid under severe contribution, but they altogether failed in yielding a satisfactory explanation of the process of absorption. The doctrines of capillary attraction and atmospheric pressure, the legitimate offspring of these sciences, are the most conspicuous. For their authors these hypotheses have procured considerable celebrity, but we are persuaded it must be temporary in its character. If their standing with posterity is to rest upon these speculations exclusively, we are sure, that their names must be consigned to an early and unenviable oblivion.

To the readers of this Journal, a detailed refutation of these hypotheses must be unacceptable, especially when they are in possession of a very able paper on the same subject, by Professor Caldwell, which is to be found in the first volume of this work. A few remarks applicable to them either separately or together is all that we shall obtrude upon the reader at this time.

If absorption is to be attributed to any peculiar mechanism over which the vital principle does not exercise an exclusive control, why is it, that absorption and exhalation are frequently not only increased, diminished, and sometimes even altogether suspended, but their products, so changed in quality, that it is absolutely impossible to recognize in them a single attribute by which in their normal state they are known, and yet be unable even to suspect a corresponding change in the means of mechanism employed by these hypotheses. These are the morbid phenomena which the physician is summoned daily to correct, and yet, we never hear of such remedies being incorporated with his therapeutics, the methodus medendi of which is to be explained upon physical principles, and such as these hypotheses, if true, would naturally indicate. Recourse is invariably had to such agents exclusively, as operate in a constitutional or local manner upon the vitality of the part diseased.

It is a notorious fact, that absorption takes place more readily where the epidermis is thin, delicate and habitually moist, as on the lips, inside of the mouth, the surface of the glans penis, &c.

Why does capillary attraction exert its energies in these places more powerfully than in others? Why does the atmosphere press upon them more potently than other parts of the body?

If the atmosphere exercises any direct agency, contrary to what uniformly obtains, we would expect to find absorption more active externally than internally. Does it press with more force upon children than upon adults; upon women than upon men; during sleep than while awake; with less in high than in low temperatures; and with less during plethora than than after the abstraction of blood? Is the body in the morning, when it is refreshed and vigorous, more exposed to the action of the atmosphere, than in the evening, when it is fatigued and exhausted? Why does a man strong and robust escape the contagion of the venereal virus, and become diseased, when he has enfeebled himself by excessive sexual indulgence? Why does a calm equipoise of unexcited feeling shield the orientalist from plague, and the agitation of fear and solicitude consign him to the grave? Do these phenomena betray any of the evidences of the direct operation of a physical cause? Candour would extract a negative reply from Dr. David Barry himself.

We are informed by the elegant author of Lord Anson's voyage, that some of the seamen had fractured bones, which had been united for many years, and had the connecting callus absorbed, and the fracture reproduced without the operation of any exciting cause externally applied. Recorded by Mr. Abernethy we have several cases of the same character precisely. Did the atmosphere have any agency in reproducing the fractures in the instances just alluded to? Did the atmosphere press specifically upon the uniting callus and no where else?

Although Dr. Barry has recourse to atmospheric pressure to explain the phenomena of absorption, it is a fact, which has not yet been established, that pressure invariably increases the action of the absorbents. Indeed, we are not altogether destitute of facts, which go to prove, in the most direct manner, precisely the reverse. In the experiments of Dr. Bouillaud it was ascertained, that pressure is the most effectual means, that can be

employed to arrest the deleterious progress of a poison inserted into a wound. The cupping glass acts precisely as pressure made by the hand, or by a ligature; and it owes its efficacy to the energy exercised by its edge upon the part, and not to the vacuum produced under it. The experiments of Dr. Bouillaud have been repeated by By Dr. Pennock and Rodrigue with a full confirmation of the results obtained by him. A pathological fact of practical interest bearing upon this point is recorded by Dr. James Johnson. Speaking of encephalic pressure he remarks "this oppressed state of the sensorium renders the absorbent system so torpid that there is no chance of mercury being taken into the constitution."

If however, it were incontestable, that pressure always promotes absorption, the production of a single unimpeachable fact showing that this process is carried on where the supposed promotive agency of pressure cannot be suspected of exercising any influence, will be sufficient to demonstrate that the ruling power ascribed to the atmosphere by Dr. Barry is gratuitous and unfounded. In the bones we are presented with an instance of this kind: in the osseous tissue numerous phenomena prove, that absorption is carried on while its hard and unyielding nature forbids the idea, that the atmosphere does or can excercise any influence. The fœtus in utero presents us with another from which an inference, equally conclusive may be deduced.

The hypotheses of those, who would establish an unnatural alliance between the physical sciences and the science of life cannot, upon any principle which has yet been disclosed, assign a satisfactory reason for the circulation of lymph in every possible direction throughout the system of absorbent vessels. To make every reasonable admission, according to their principles, it is very manifest, that it is impossible for the lymph to be elevated above a certain fixed point, or in any other than one specific direction. A candid examination, therefore, of these hypotheses must convince the physiologist in the earnest pursuit of truth, that, however, close and striking may be the analogies deduced from the physical sciences, they are still of such a character as to be altogether incapable of explaining the phenomena of life.

The numerous and manifest changes produced in the substances submitted to the elaborative action of the absorbent vessels, is positive and irresistible evidence, that absorption is a process purely vital in its character. The condition of the products of this action in diseases as well as in health clearly reveals this truth. The existence in the vessels of this system of the multiform products of abnormal action cannot be explained upon any other principle. To no other tenable cause can the numerous varieties of the function of absorption, which correspond with so much precision to the different developements of this system, so characteristic of the various epochs of life, be with any appearance of reason ascribed. These are among the most conspicuous phenomena, which distinguish animate from inanimate matter. They disclose remarkable energy on the part of these vessels in the execution of their functions, which is ever opposed to the passiveness implied by the hypotheses of the mechanical and chemical physiologists.

If, indeed, it were true, that the absorbent vessels are passive agents, and that they circulate their contents upon physical principles, the chyle, lymph, &c. should exist already formed previous to their absorption. That this is untrue, the evidence is so multiplied, unimpeachable and concurrent, that it would be a work of supererogation to adduce it. There is no relationship or affinity between the results of dead matter and those of the elaborative actions of life. Between the materials acted on, and the results of this action, there is no physical and frequently no chemical likness. The food is converted into chyme by the elaborative action of the stomach; the chyme into chyle by the elaborative action of the lymphatic absorbo-exhalents of the bowels; the chyle into blood by the elaborative action of the respiratory apparatus; and the blood into fibrin and phosphate of lime, saliva and gastric juice, by the elaborative action of the arterial absorbo-exhalents. Can the physical sciences furnish a satisfactory explanation of one of these remarkable transformations?

The ascription of the process of absorption to an action of vitality exclusively we are told, reflects no light upon the nature

of this function. We still remain in utter ignorance of its intimate nature. This we never can comprehend, unless we understand the nature of the principle of life itself. Illustrious instances of the extreme folly of searching after this essence are familiar to those acquainted with the history of philosophy. To speculate on this subject, would be to soar into the regions of lawless conjecture and fable, where the imagination would be lost and bewildered in the inextricable mazes of its own creation. To ascertain its laws must of necessity constitute the *ne plus ultra* of the attainments of the human mind.

One truth we consider established on a basis as immutable as the everlasting decrees of gravitation; which is, that the principle of life is radically unlike every thing, that has been discovered in dead matter; that the laws which govern the one wage a war of extermination against those that regulate the operations of the other. Notwithstanding they are thus dissimilar, there are those, who more familiar with the nature of dead than of living matter, exclaim against those, that would advance our knowledge of the laws of the latter, without calling in the aid of the physical sciences, *speculation! speculation!* If the charge be disreputable, those by whom it is preferred, appear not to know the extent to which they compromise their dignity as philosophers. If it be a crime to attempt to comprehend the laws of life by a classification of the facts presented by living matter, how much greater is the magnitude of the offence of those, who attempt to comprehend them, by deductions made from the phenomena presented by dead matter? It is an effort to unite that which Deity has declared to be separate and distinct: for the laws of dead matter never commence their work of destruction until those of living cease to exist.

The annals of fancy does not furnish us with an instance of more daring, wild, Utopian and unprecedented speculation. Less substantial than the visions of the alchymist or the reveries of Descartes, it should be classed with those durable monuments of human weakness to be found scattered throughout the darkest ages of ignorance and superstition.

As easy would it be, to unite oil and water, ornament and

mathematics, as dead and living matter: and if it be "an evidence of intellectual imbecility," according to the aphoristic declaration of a Grecian sage, "for a man's exertions to exceed his means," how low must that order of philosophers stand in the scale of mental excellence, who contend that the economy of animals is nothing more than a chemical laboratory?

[TO BE CONTINUED.]

ART. III.—*A History of the Climate and Diseases of Tennessee.* By LUNSFORD P. YANDELL, M. D. of Murfreesborough Tennessee.

SO far as the writer of the following paper is acquainted, no detailed account of the climate and diseases of Tennessee has ever yet been published. It affords him pleasure to know, that hands are to be found in the State more competent to the task than his; but as these have never been applied to it, he has determined to contribute what little he can towards its accomplishment; and as the harvest will be by no means exhausted, others may follow in the same track, who, with more enlarged experience and greater skill, will labor with a better prospect of success. He makes no pretensions to originality of views or practice. It is even probable that no new principle of pathology or treatment will be found developed in the following pages. But he hopes, that the facts which he shall be able to unfold may, by shedding some additional light upon the character of our febrile complaints, bring us nearer to certainty and the truth; and that his paper may be of some avail to the author who shall, at a future day, attempt a systematic work on the diseases of our continent.

Tennessee extends from N. lat. 35° to 36° 40', and in long. from 4° 12' to 13° 14' W. from Washington City. Its area, measur-

ed by the rhumbs, is 43,265 square miles. Its mean length from E. to W. is 400 miles, and mean width 108. Bounded by the Mississippi W., State of Kentucky N., Virginia N. E., North Carolina E., Georgia S. E., Alabama S., and the State of Mississippi S. W. It is naturally divided into three great divisions;—the eastern, which is mountainous; the middle, undulalating; and the western, generally level and partaking of the character of the Mississippi bottoms—presenting the aspect of an inclined plain, which extends from the last ranges of the Alleghany mountains to the Mississippi river, embracing three or four degrees of temperature, and almost every variety of soil and vegetation.

The soil of E. Tennessee is of secondary formation—less fertile than that of the middle and western portions of the State, but more productive than its appearance would indicate, on account of the great proportion of carbonate and sulphate of lime which enter into its composition. Less adapted to cotton and Indian corn, it brings wheat and apples to greater perfection, than the other divisions of the State. Its principal rivers are Holston, French Broad, and Clinch; which, rising in the midst of mountains, have precipitous banks and rapid currents; are subject to great overflows, rarely stagnate, and never cease wholly to run. They are remarkable for their pellucid, and healthful-looking waters. The greater elevation of this end of the State makes it colder by about 2°, than the middle region—four hundred feet being found to produce a variation in temperature, equal to 1° of Fahrenheit. The flora of E. Tennessee, as well as thermometrical observations, shew this difference. This is the region of romantic scenery, of minerals, caves, cascades and health.

Middle Tennessee, though less mountainous than East, is still undulating—has a more tropical climate, fresh, ever-running streams, and a soil, generally, of great fertility. Its principal streams are Cumberland and Tennessee rivers. The banks of its water-courses are less precipitous, and a wider surface of country is inundated by the vernal freshets. Collections of standing water, constituting ponds and marshes, are

more common, and many of the smaller streams cease to run during the droughts of summer. With these, we have a corresponding increase in bilious disorders. East Tennessee is accounted salubrious, compared to this quarter of the State.

West Tennessee presents the aspect peculiar to all the countries bordering upon the lower Mississippi. The soil, a great portion of it, is of alluvial or ternary formation; the streams are deep and sluggish, their banks low, and subject to inundation during the spring tides. Its extended plains, contiguous to the streams, present, during the wet season, the appearance of extensive inland lakes. Lying farther to the South, and 800 feet below the level of the Cumberland mountains, the temperature of this quarter is naturally higher, and its flat surface deprives it of the refreshing breezes, which relieve the other divisions of the State. Earth-quakes are common here, produced, probably, by the falling in of the crumbling banks of the Mississippi. Moschetoes, and every species of tropical insects abound. Thunder storms are frequent. The Forked Deer, Big Hatchee, and Obion, the principal streams, have a fall not exceeding three or four inches a mile, and move their sluggish waters on to the mother of streams with a current scarcely perceptible. Such fruitful sources of disease make an annual exhibit of their force. It is here that, of all the parts of the State, the autumnal diseases are most regular in their appearance, and most malignant in character. Not so fatal as those of Louisiana, Mississippi, or, perhaps Alabama, they require a much more prompt and energetic treatment, and are much more extensively destructive, than those of East or Middle Tennessee. Jackson, the chief town, which is situated near to the Forked Deer, and in the neighbourhood of an extensive marsh, has been severely scourged by fever. In the fall of 1823 it was peculiarly fatal.

Nashville the metropolis of the State, on the banks of the Cumberland, is healthy. It is built upon a rock, whence the rains are soon carried into the river or evaporated by the sun; and as it is contiguous to no marsh, the effluvia which create fever are not generated in any considerable quantity. A case of yellow fever is reported to have originated there, several years ago,

from a large mass of putrefying coffee. It was isolated, and if genuine, the only such case that ever occurred in the city.

Columbia, the second in size, of the towns of Tennessee, situated on Duck river, has had occasional visits of the fever of a very malignant character. A great proportion of the population of the place were sick, in the autum of 1823, and many died.

So much for the internal configuration and peculiarities of Tennessee. Midway between the Frozen Ocean and the Gulf of Mexico, it has a climate of medial temperature—comparatively free from the extreme cold of Ohio and the sustained ardor of Louisiana. Excepting, however, the diversities produced by relative position, it has the climate common to all the States of the great Mississippi plain. Unsheltered by any mountain barrier from the winds of the North, these have a full sweep from the polar sea to the Gulf of Mexico, and whenever a vacuum occurs in the heated atmosphere of this latter region, the heavier air of the North pours down the valley to fill it up. As this must happen very often these winds prevail occasionally at every season, with the effect always of depressing our temperature. Between the North and the South there seems to exist in this respect, during a good part of the year, but especially the spring and fall, a ceaseless struggle. With the prevalence of the South, we have warm weather, even in winter—of the North, in winter, extreme cold; and in summer, such, as frequently to render fires comfortable in June. When summer has fairly set in, it is nearly as warm in Boston or even Quebec, as in New Orleans or Charleston—the difference of a Northern and Southern summer, in the United States, consisting rather in duration than in intensity—and then the heat becomes steady and uniform. While it is yet winter to the North, spring is far advanced in the South; and as we have our winds from one quarter or the other, we have spring or winter. Thus the hope of our harvests and fruits is frequently blighted by the prevalence of the North-west wind when the season is far advanced. Killing frosts have occurred in Tennessee as late as the 10th of

May. This is at once the distinguishing feature, and the infelicity of our climate.

Between the lakes to the North, and the Gulf of Mexico to the South, Tennessee is amply supplied with rain. The rainguages that have been kept, shew about 45 inches a year, which is more than one third more than falls in Europe, 5 inches more than falls in Pennsylvania, (owing, probably, to the great flood of the Mississippi which, during the vernal overflow, spreads out like an inland sea, and saturates the atmosphere with moisture) and about that much less than falls in New Orleans. As the forests are cleared, and a greater proportion of the surface exposed to the sun, by which the humidity of the soil is diminished, the annual fall of rain is becoming gradually less. But that happy feature in the physiognomy of our country, the approach of the two great chains of mountains as they stretch to the South, will forever secure the Mississippi valley against the mischievous effects of severe droughts. These have been happily compared to guardian giants, which raise their mighty arms to collect the vapours and disperse them in fructifying showers, over the valley committed to their care.

It was the opinion of Count Volney that there was a mysterious mildness in the climate of the Mississippi valley; and Mr. Jefferson believed it to be several degrees warmer than a parallel latitude on the Atlantic coast. Mr. Darby, Dr. Drake and Mr. Flint hold, justly, a different belief. Climate is not always the same in the same latitude. It is affected by mountains, large bodies of land, and collections of water. The former diminish, the latter give uniformity to temperature. The west side of the old Continent, as France, Spain, Portugal, &c. is humid and mild from the prevalence of Western winds moistened and tempered by the lower and more equable temperature of the Ocean; while Poland, Russia &c., are colder from the winds blowing over a more extended tract of land. So our Pacific coast is moist and mild, from the contiguity of the Ocean, over which the west winds reach the shore. As we approach the Mississippi Valley, travelling to the East, where the winds have swept a great extent of continent, the climate becomes

more cold—at least 10°, Mr. Darby avers, on the same parallel—and finally it is again moderated upon the Atlantic slope, from the proximity of the ocean, and especially the Gulf stream. The free course and general prevalence of the North-west winds aid these causes in depressing our temperature. The progress of cultivation will affect our climate in some respects. While the clearing of the forests, by exposing more of the earthy surface to the action of the sun, will render our atmosphere drier and more comfortable, both in summer and winter, it will also render vicissitudes easier and more frequent as it removes obstructions to the wind. It is believed that the felling of the forests in Alabama and Mississippi, which has been done in the last few years, has increased our proportion of South and South-west winds.

The snows which fall in Tennessee, becoming gradually fewer, all come from the North, or North-west. A large proportion of its winds are from the North-west, and South-west—its rains from the same quarter.

The mercury in Fah. thermometer has fallen as low as 12° below zero, and has risen as high as 98°—producing a range of 110°. In January 1823 it fell thrice 6° below zero, and the same thing occurred once in 1826.

The periods of intense cold are rare and of short duration. Streams of large size never freeze over. We have not seen Stone's river, which is a small stream, frozen over so as to bear a man's weight, in twelve years. The heat of summer is more intense. During a drought of more than three weeks in the summer of 1825 the thermometer ranged from 80° to 93°, only falling to the lower point during the night, and one or two days, it rose as high as 98°. This spell prevailed in August during the visit of the late comet, and is without a parallel in many preceding years, or the time that has since elapsed. It was destructive to many kinds of vegetation; the crops of maize and cotton were much shortened. It is perhaps the only season in the recollection of the oldest inhabitants, when the breezes brought no refreshment.

The winter of 1823–4 was uncommonly warm. But two falls of snow occurred during the season, which were in February:

there was no continuance of cold over two or three days at a time. The spring, notwithstanding, was backward, warm weather, not setting fairly in till towards May. May and June were uniformly humid and warm. Fever, cholera and dysentery appeared early, and were obstinate. July was hot and dry, the thermometer from 80° to 90°; and in this month, fevers and bowel complaints were greatly increased in frequency and malignity. These disorders continued to prevail till arrested by the frosts.

The winter of 1824-5 like the preceding, was remarkable for its mildness, and was likewise dry. The spring was warm, and forward, and also dry. No rain fell in April for several weeks. May too was dry and warm; the thermometer ranging from 81° to 86°, in the shade, during the day. Some cases of cholera infantum occurred about this time. In June there was no fall of rain for 21 days. Dysentery, of a highly inflammatory character, made its appearance in some neighbourhoods. But little rain fell in July. In August the unexampled drought occurred, of which we have made mention; and though a period of such excessive heat, it was salubrious. The complaints which had begun to spread in the preceding months, disappeared during its continuance. Except in peculiar situations, the country was uncommonly free from disease. The family of J. R. was attacked with fever. He lived on the west bank of Stone's river, which, generally a bold stream, had become low from the severe drought. The wind had blown several days from the east directly across the river upon the house. Nearly the whole of the family suffered. This case is the more worthy of remark, as it was the only time that fever, in more than ten years, ever appeared in the family of Mr. R. and is, probably, the only instance in that time of the winds blowing from the east, when the river was so low. Dysentery, during this spell, was more rife, and more intractable than fever. The autumn continued dry and healthy.

The winter of 1825-6, though colder than the preceding, was moderate. A few periods of intense cold occurred, in one of which the thermometer fell as has been mentioned, 6° below zero. Influenza spread, during this season, from one end of the

continent to the other. Commencing in Boston in January, it reached Tennessee in February, and continued to prevail till about the first of April. The spring was wet, the summer humid and mild. Fever prevailed extensively, but was of a mild character.

The winter of 1826-7, like the preceding, was moderate, with but a few very cold spells. The summer was dry. Fever did not spread extensively, but sporadic cases were obstinate.

The following winter was one of unvarying warmth, and almost ceaseless rain. The wind was ever from the W. and S. W. The warmth and humidity of the atmosphere made it almost impossible to cure pork. Water froze but three or four times during the season. The spring and summers proportion of rain fell in winter, and a drought, almost without an example, was the consequence in the spring and summer. The season was generally healthy. When fever appeared it depended on local causes, and did not spread. It visited the family of J. H. in a peculiarly malignant form. Twelve or fourteen members of the family suffered severely, and two died. Mr. H. lived near to a pond of considerable extent, which had never become dry before this season. In addition to this circumstance, a forest between the pond and house had been cut down, in the preceding winter. This gave the effluvia a fair sweep to the house.

The winter of 1828-9 was the *e contra* of the preceding. One of unvaried cold and dryness. The thermometer never fell very low, but the ground continued unthawed for several weeks; which is an uncommon circumstance, in our climate, when the S. W. and N. W. winds succeed to each other in such rapid alternations. The spring too, was dry, cold and backward. The summer was rainy, but the country was at no time drenched in rain. The ponds were not surcharged, nor did the streams overflow. Rains fell moderately and regularly. The streams did not stagnate. The temperature was never very high. This was a season of general health.

East Tennessee, however, suffered uncommonly with bilious disorders—more, perhaps, than during the ten years that preceded it. This fact is in accordance with the general experi-

ence on this subject—that mountainous regions are healthy in dry, and sickly in wet seasons; while low, flat countries are healthy in wet, and sickly in dry seasons.

Thus, upon a review of past years, we find that our bilious complaints are regulated by the amount and distribution of heat and moisture. The spring, and first summer month of 1824 were wet and warm;—July was warm and dry—This was a sickly year. The following winter was warm and dry; so was the spring and the summer. This on the contrary was a healthy year. The winter of '26 was wet—rains were abundant in the spring, and fell frequently through the summer. The heat, the mean time was moderate. Fever this season spread over the whole country; but it was mild and manageable. The spring and summer of 1827 were dry, and were followed by a salubrious autumn. The drought of 1828 commenced in May; the summer was dry throughout. The fall of this year was likewise healthy. A cold, uniform, dry winter, the next year, was followed by a cool, dry spring, and a mild, humid summer. A season so favorable to vegetation has not occurred for many years. Crops of Indian corn were singularly abundant. And yet this was a summer and autumn of health.

From all of which it is seen, that the sickness of our climate is in proportion to the rain during the spring, and first month of summer. If these are dry, so that the surface of the earth loses its moisture the streams run down, and the ponds become evaporated before the extreme ardor of summer, there is but little sickness. If, on the contrary, the spring, and early summer months are humid, fevers abound in July and August, and the autumnal months. The warm season of the year 1817 was remarkable for its humidity, and high temperature. Thunder storms were frequent. It was perhaps the most sickly season that has been experienced in modern years.

Diseases.—Those diseases of all countries and all climates—measles, consumption, whooping-cough, the phlegmasiæ &c., are common to us with the rest. We have but little gout; small-pox has never spread in the state; scarlatina is a rare complaint; and consumption is less common than in the cities of the sea

board; but our variable climate is a too fruitful source of this. Dyspepsia is as fashionable here, as in those countries where the indolence and luxury of civil life would seem to designate its appropriate abode. There may be something to favor this in the miasmatic character of our atmosphere, as well as in the diet of our population, which is composed too principally of animal food.

Influenza has occasionally appeared under a most malignant form, and with a fatality unknown to our other complaints. The winter of 1815-16 will long be remembered for the ravages which this disease, otherwise called *pneumonia typhoides*, then committed. Almost the whole population seemed, at one time, to be laboring under its effects, and in some instances it swept off entire families. Dr. Hogg, an experienced physician now of Nashville, acquired great reputation for the ability and success with which he treated it. Viewing it as a disease of congestion, he resorted freely to the use of venesection and cathartics; and his success was probably equal to that of any physician in the United States, who had an opportuniiy of practising in the disease. To this practice, the late Dr. Wilson Yandell, who saw much of the complaint, added, with good effect, emetics. The late Professor Davidge, of Baltimore, was in the habit of declaring to his class, that this plan (*the depleting*) was attended by the most fatal consequences in that city: so true is it, that the character of diseases is modified by climate, modes of living, &c., and requires a corresponding diversity of treatment.

Cholera Infantum is the scourge of our infant population. This annually tithes us. A great proportion of our children suffer more or less with it, during their second summer. In the country it is not, however, a formidable disease when met early with the proper treatment. In towns it is more serious, for there the constitution seems unable, from the loss of recuperative power, to repair the ravages committed by the disease; and after the the acute stage is past, the patients often sink under debility, or from the effects of chronic inflammation. This, however, oftener depends upon a want of energy in the early treatment of the complaint.

From our experience in this disease we feel authorized to say, that almost every thing depends upon the promptitude with which it is met, and the skill with which the depleting remedies are administered in its early stage. This, parents should have (*line upon line and precept upon precept*) repeated every day in their ears. If they have temporized with astringents, or their own favorite domestic remedies, until the disease has produced disorganization of the delicate mucous membrane of the stomach or bowels, it is in vain that they seek the aid of a physician. The golden moment, when he might have administered relief, is now past. Venesection is generally called for in the treatment of this complaint. If children bear it less well than adults, their diseases are characterized by high excitement, and running their course rapidly, do irreparable injury, unless promptly arrested. We have found the disease to yield sooner when general blood-letting has been resorted to at the commencement of the attack. Topical bleeding is of great value, and *leeches* afford the best method of effecting it. An opportunity of applying these should never be pretermitted when the case seems to require them, for besides being less disagreeable to the patient, they are less obnoxious, than many internal remedies, to the prejudices of the parents; and where there is much inflammation in the *primæ viæ* attended with high fever, irritability of the stomach, discharges of blood, &c. they afford great relief.

After general and topical blood-letting, *calomel* is our main dependance. We generally give it, in doses of from one to ten grains suited to the age of the patient, once in twenty-four hours; and when the case is alarming, as often as three times a day. The object is to allay the irritability of the stomach, and restrain the sanguineous discharges, and the calomel must be given till this is accomplished. We have not however, found any great amount of this to be necessary, when application has been made in time. A few doses will generally restore the secretions, and with them health. Opium, in the form of the *Chalk julep*, is an article of value in this complaint. Without increasing the fever, or counteracting the operation of the calo-

nel, it mitigates the distressing tenesmus, frequently checks vomiting, and adds much to the comfort of the patient.

Emetics are given by some physicians, and may be beneficial when the complaint is chiefly seated in the lower part of the alimentary canal; but when the stomach is much affected they are of very doubtful efficacy. Blisters, unless it is towards the close of the disease, are still more *anceps remedium*. After what we have witnessed from their application we should be loth to use them except as a dernier resort. Still, in the last stage, when the patient has become greatly exhausted, with a pale sunken countenance and cold extremities, and when it would seem hazardous to push depletion farther, they are called for, and occasionally exert a most salutary influence. We have seen a favorable crisis given to the complaint, under circumstances like these, when the case seemed almost hopeless. Applied earlier, while the febrile action is yet unsubdued, they invariably increase the excitement, and frequently create a local disease as alarming, and as intractable as the original complaint. We have seen, as the consequence of their too early application, serious hæmorrhage followed by gangrene of the part. By the plan which we have indicated, opportunely used, and assisted by a proper diet, without which all remedial efforts are vain, we have generally been able to conduct the case to a favorable issue.

The *Cholera* of adults, though a rare disease, has sometimes proved fatal in this country. It should be treated with *tinct. opii.* till the vomiting is checked, and then with mercurial cathartics

Diarrhœa is a mild disorder, rarely requiring the aid of a physician.

Dysentery is a much more dangerous disease. In proportion to the number attacked, more perhaps die of it than of fever. Blood-letting is of primary importance here; for although less effectual in inflammations of the mucous than of the serous membranes, it increases the virtue of every other means. This is to be followed by mercurial cathartics. These must be depended upon to restore healthy secretions, and nothing will be found

to mitigate the incessant tormina like them when given till the character of the evacuations is changed. Dover's powder relieves griping, lowers arterial action, and induces perspiration, by which the ease and comfort of the patient are equally promoted. Blisters are sometimes required. Fomentations to the abdomen, and enemata of starch and laudanum, afford temporary relief. Emetics, especially of ipecacuana, have occasionally been found of peculiar efficacy.

Fever—In a former part of this essay we have seen that fever obeys the seasons; but whatever be their character, we never fail to suffer more or less extensively with it. When they are wet in their early part, and dry and warm in summer and fall, we have seen that the disease abounds; when there is but little moisture to be exhaled during the hot season, it is less prevalent. Hence we infer, that fever grows out of causes which have their origin in a high temperature and a humid atmosphere, which afford facility to rapid chemical changes in vegetable matter.

The character of our fevers is generally that of open excitement; sometimes it is inflammatory; rarely congestive. Our mild climate does not eliminate the causes in a sufficiently concentrated state to produce this latter form. The powers of the system are not often depressed beyond the point of reaction. This however applies especially to that part of the state which has formed the field of our observation; in that portion which borders upon the Mississippi congestive cases are not so rare in their occurrence.

The theory of Armstrong, relative to the pathology of fever, held, until recently, almost entire dominion over the minds of the physicians of this state. *Broussais* is beginning to divide professional opinion with him. The truth here, as in most other cases, lies probably, between the extremes. All the symptoms of fever, in ordinary cases, indicate to our mind, the existence of congestion in the great veins of the interior. It is this which, in our view, gives rise to the different stages of the disease; creates the head-ache, the pain in the back, the fulness in the epigastrium, and right hypochondrium; causes the coldness and shrinking of the

surface; and, finally, which suspends the secretions. What the previous change in the nervous system is, which causes this failure in the functions of the body, it is difficult in the present state of our knowledge to tell; but it is probable that some læsion here constitutes the first link in the chain of disease.

On the other hand, we freely give our assent to the doctrine, that the mucous membrane of the stomach and bowels is often inflamed in fevers. This too is evidenced by the symptoms; the tenderness of the epigastrium; the appearance of the tongue frequently; the extreme irritability of the alimentary tract; &c. Congestion always exists when there is inflammation, and very often terminates in inflammation. There is congestion of the lungs, in pneumonia; of the pleura, in pleurisy; of the cartilages, in rheumatism; of the brain, in phrenitis, &c. In fact inflammation never exists without congestion. Congestion, on the contrary, comes on, and after continuing for a time may subside, without inflammation. This we believe is often the case in fevers of short duration, when the head is mainly affected, the excitement considerable, without any well marked symptoms of disorder of the alimentary canal. It is practicable we should think, to discriminate between such cases as depended upon inflammation of the mucous membranes; the gastro-enteritis of Broussais; and those which have their origin in congestion; and the discrimination might lead to a salutary diversity of treatment.

The *modus operandi* of marsh miasma we deem yet a point of great obscurity. Professor Cooke, who has written very recently on the subject, believes that it acts on the blood in the shape of *carbonic acid gas*, and poisoning it thus weakens the action of the heart, and brings on the disease. Many suppose that it is taken into the stomach with the saliva, and there makes the first morbid impression from which all the symptoms spread out as radii from the centre of a circle. Others, that the morbid impression is first made upon the lungs, &c. If it were important, and our duty, we have no hope of being able *lites componere tantas*. The *methodus medendi* is a subject of much higher import; and here too we shall hasten on, without stopping to reconcile conflicting views, or to expose the error of any system;

our object being, not to find fault with others, but to lay before the profession that practice which, in our hands, has proved most safe and efficacious.

Venesection, as it should precede all other remedies, may be first mentioned. In our hands this has proved a remedy of the most unequivocal value. We have frequently had cause to witness the worst consequences from a neglect of it, but never saw it do harm, when opportunely resorted to. It not only subdues febrile excitement, wards off local determinations and hastens the subsidence of the disease, but so removes all congestion as to obviate those *sequelæ* of fever which often prove more ruinous than the primary disorder. The blood should be drawn in a full stream, and, when the case is bad, *ad deliquium animi*. It loses more than half its efficacy by being drawn in quantities so small, as not to make a decided impression upon the system. We always expect to find our patient, in a few hours, much relieved of head-ache, &c., with a softened pulse, a relaxed surface, and greater susceptibility to the impression of medicines, after free venesection, and are rarely disappointed. *Leeches* as a safe, elegant, and often efficient remedy, deserve the attention of the practitioner, and promise to do much good when inflammation or topical congestion exist. In cases where the determination is chiefly to the brain, and this organ continues to suffer after the general action is overcome, the application of these to the temples is attended with the most decided relief. They may often be applied advantageously to the abdomen also; to whatever part, in fact, that appears to support the violence of the disease. As it is sometimes important to gain the confidence of the patient, as well as of the friends, we know of no remedy which comes in better, *ad captandum*, than leeches.

Calomel.—This is the favorite remedy of the physicians of the South and West. Those who have had most experience in the use of it are most confident that it is the chief reliance in the fevers of our climate. With us it has proved every thing that we could expect of it. It has gone before every other article in establishing healthy secretions, without which it is impossible to cure any disease. It has removed the intolerable oppression which in bad ca-

ses is felt about the præcordia, promoted the discharge of bile, allayed nausea, and obstinate vomiting, and, finally, effected a favorable crisis when all other remedies had proved unavailing. But as it is one of the most powerful of our means, so it should be used with the greatest discrimination. It has been made a serious question, whether, administered as it has been, it has done more good or harm to our race. There can be no doubt, but that it has slain its thousands in the hands of the ignorant. We have heard of its being given to the amount of *six hundred grains* without purging, and the patient died of course. It is sometimes persisted in too long, and not sufficiently aided by other cathartics. Nevertheless, it is the duty of the intelligent and honorable of the profession to prevent it from falling below its proper value, and to guide and subdue the current of public prejudice, which, at all times, has been found ready to rise up against it. Without calomel, the profession would be Sampson *shorn of his locks*. This much we have thought excusable to say on this subject, because if, as we think we see indications, public opinion is about to react, calomel, from having been, perhaps too indiscriminately used, may fall into unmerited disrepute. This has been the fate of all potent remedies in every age of the profession.

We have not found it necessary to give it in exorbitant doses, nor to repeat it very often. From ten to twenty grains at bed time, and assisted by some other cathartic next morning, we have generally found sufficient. We prefer to give the calomel alone, for by passing more slowly through the alimentary canal it does more good. The impression which it makes upon the liver and other chylopoietic viscera is greater, and a greater discharge of bile &c., is the the consequence. If the bowels are not moved by the calomel, rhubarb, aloes, jalap or scammony, or a combination of these may be administered, to hasten its operation. Any of these articles promise more as adjuvants than the *saline cathartics*, which are apt to promote watery discharges. It will be found as has been clearly put down by Rush, Armstrong, Cooke, &c., that convalescence is never perfectly established until the alvine evacuations have become copious, dark, and con-

sistent. With these, the disease begins to yield. They must be kept up by means of the mercurial cathartics till the paroxysms of fever cease to recur.

Emetics.—In the practice of many physicians these are a prominent article. Dr. Cartwright seems to lead the way in the boldness of their administration. The late Dr. W. Yandell used them extensively and with success. Perhaps few physicians cured a greater proportion of their patients. Adopting the maxim of Rush,—that 'emetics meet the thief at the door and drove him back, while cathartics, in driving him through the house, give him an opportunity of doing harm'—his habit was to prescribe an emetic next after bleeding. He thought that, in this way, he was able to cure his patients quicker, and with greater certainty. We have not had much experience in the employment of them. Tartar emetic, in small doses, so as to nauseate gently, has a fine effect in relaxing the skin, and diminishing arterial action. The *pulv. ipecac. compos.* may be made to answer a valuable purpose in the same way. As a diaphoretic, this has proved superior, in our hands, to all other articles. Nothing induces perspiration with so much certainty and at the same time quiets restlessness and composes the system so well. When there is much determination to the head it may be improper; but in those cases in which the complaint has its seat chiefly in the organs of the abdomen or the chest, its employment is attended with the most benign effects.

Cold Affusions.—Under the circumstances laid down by Dr. Currie, these have proved, with us, as valuable as they are grateful to the feelings of the patient. Cold water as a drink, and applied more or less extensively to the surface of the body, may be resorted to in almost every case of fever, and with decided advantage. Nothing is more congenial to the feelings of the sick, and nothing diminishes more the pungent heat of the surface, and the morbid activity of the heart and arteries. It generally brings on a gentle diaphoresis, allays irritability and restlesness, relieves head-ache, and induces sleep. We had a patient during the last season, whose fever continued to recur every day, in spite of the most copious depletion, with such

violence as to create delirium. The cold bath was used with prompt and decided benefit. The head-ache was removed, he returned to his senses, the fever abated, and never recurred afterwards. This is a single case out of a multitude. Dr. Smith in his treatise on typhus, mentions the cold bath as the only remedy which, with him, ever cut that disease short. Poured in a stream upon the head, when not deemed advisable to apply it to the whole body, it is agreeable and useful. Dr. Gregory considers it a valuable remedy in Hydrocephalus, when used in this way.

But this, like all other strong remedies may be abused. We have known cases in which, used at too late a stage, its influence was decidedly prejudicial—producing a fatal collapse of the vital powers. Much judgment is necessary in directing the use of a remedy of such efficacy. The rules by which we have been governed are those of Dr. Currie, which as they are of great importance, and not long, we take the liberty of transcribing.

1. "It should never," he observes, "be used when any considerable sense of chilliness is present, even though the thermometer, applied to the trunk of the body, should indicate a degree of heat greater than usual.

2. Neither ought it to be used, when the heat, measured by the thermometer, is less than, or even only equal to the natural heat, though the patient should feel no degree of chilliness. This is sometimes the case towards the last stages of fever, when the powers of life are too weak to sustain so powerful a stimulus.

3. It is also necessary to abstain from the use of this remedy when the body is under profuse sensible perspiration, and this caution is more important in proportion to the continuance of this perspiration."

Blisters. These are often beneficial in the latter stages of fever, where after the general violence of the disease is subdued, local affections keep up the morbid excitement. Under any other circumstances, they do not appear to us to be indicated. In the early stage of the complaint they add to the febrile excitement without any compensating benefit. They have been

recommended in intermittent fever, and applied an hour or two previous to the access of the chill they are sometimes effectual in checking the disease. But in the bilious remittent fever, to which all our remarks in this paper are intended to apply, they are never called for unless local congestions or inflammations have occurred.

Sinapisms may be had recourse to at an earlier stage, and by equalizing the heat and circulation, often do good. Applied when the cold fit is felt to be approaching, they sometimes ward it off, always shorten it, and rarely fail to moderate the violence of the succeeding stage. They contribute greatly to the comfort of the patient, and ought not, therefore, to be neglected.

Tonics. These once occupied a high rank in the curative systems of writers on fever; but in our day, they have fallen into comparative disrepute. They are now rarely deemed necessary in remittent fever, when the case has been well treated, and there is ordinary integrity of constitution. . Until the pyrexial stage is past, they must do positive harm, and when the system has been judiciously conducted through this, the unaided vital powers are generally adequate to the task of restoration.

In intermittent fever tonics are indicated. They diffuse the excitement, postpone the chill, and ultimately destroy the chain of diseased action. We prefer the *sulphate of quinine* to all the other articles of this class. In doses of from one to five grains every two or three hours, during the apyrexy, it will scarcely ever fail to stop the chills in a few days. When we have succeeded in changing remittent into intermittent fever, the quinine should be administered in the same way. All the other preparations of bark are more unpleasant, and less efficient than the sulphate of quinine.

Thus, in as few words and with as much directness as possible, have we pointed out the course of treatment which we have pursued successfully in fever. Following this plan, we have not often been disappointed.

The fevers of Tennessee, like the climate, are mild. Except in the western portion of the state, where they are more malignant, they are rarely fatal unless when neglected or misman-

aged. The cases which occur in winter assume the typhoid type and are more tedious, and difficult of cure. In these cases there is a stronger tendency to local determinations, the excitement is less open and diffused, and unless well managed, have oftener a fatal termination.

The prognosis, in our fevers, may very uniformly be favorable. A crisis generally occurs on the *eighth* day; occasionally not till the *fourteenth*; and in a few extraordinary instances, the disease runs on to the *twenty-first*. In some cases, when it is about forming, the nose sometimes bleeds, and in females, the catamenial discharge comes on. This latter circumstance sometimes gives alarm, but ought on the contrary, to be regarded as a favorable symptom.

The theory of Hippocrates with respect to western climates is true, when applied to ours, only in part. "Every nation," says he, "exposed to *western winds*, has an unwholesome climate, the waters which they drink are not limpid, because the morning fogs mingle with them before the sun has had time to dispel them. Secondly the inhabitants of these places are exposed to sudden changes of temperature; for in the summer mornings, cool winds blow, and dews fall. In the afternoon, heat torments them; they have a pale complexion, and a feeble constitution. Breathing always a thick and unwholesome air, their voice becomes rough and hoarse. The West presents to us the image of autumn; the people who live under this climatological constitution, should participate in the maladies of the inhabitants of the North and South."

As, in the progress of cultivation, our diseases are coming to abound less, they are losing also much of the severity of their character. The early practitioners of Tennessee found them much more intractable than they appear now. This amelioration is owing to a greater surface of the country being exposed to the sun; in consequence of which, it is less damp; less vegetable matter is in a state of decay; the air has a freer circulation and is less humid; there are fewer marshes, and the streams are exhausted earlier in the season, and present a smaller body of stagnant, putrefying water to the sun in the heat

of summer. Our people have not a pale complexion and a feeble constitution. Nor is there any probability that we shall ever again experience diseases of so malignant a type, as those which ravaged the country at its early settlement. On the contrary, our climate will go on to improve for years to come; and when our population shall have become more temperate, and less gross in diet, we may anticipate its fruits in improved health, and multiplied instances of longevity.

ART. IV.—*Observations on a case of Disposition to form Remarkably large Cicatrices.* By JOHN GORRIE M. D. of Abbeville, South Carolina.

MORBID affections of the skin have been in all ages, from the disgust and dread with which some of their species have been viewed, subjects of interest to mankind; and they would seem, from a variety of causes, destined always to continue objects of embarrassment to the medical practitioner. The researches of science, and the results of practical observation fail alike to reduce to order the confusion in which the characteristic distinctions of a considerable number of those complaints are involved. Presenting under the operations of different habits, climates and other occurrences, a greater variety in appearance than any other class of diseases, they have in consequence, produced, among writers, more conflicting views of their character—mere individuals, existing in different countries, of the same form of complaint have been classed as distinct disorders, while maladies possessing no common connexion have been considered of the same or a similar nature. This discrepance of opinion is scarcely less apparent in the descriptions of strictly endemial complaints; and in parts of the world in

which an uniformly hard fortune leaves little room for diversity of habits, and where the authors have dwelt in situations, and under circumstances the most favourable to the study of cutaneous diseases.

In studying this complicated class of diseases the American physician has to encounter the obstacles common to the medical world, and some which are peculiarly his own; and it is therefore, not intitled to be a matter of wonder that the generality of them should have scarcely "a moderate knowledge of this perplexed and intricate subject." Resulting, as they generally do, from the association of misery, poverty and filth, evils from which our political and moral state have happily in a great degree exempted us, they are in this country, of comparatively rare occurrence. The same influences which have lessened their number have also operated, in those cases that occur, to mitigate their violence; so that they are not often made the subjects of professional advice; and with those that, from their severity or loathsomeness, become objects of medical care, it falls, from their extreme rarity, to the lot of but few physicians to be practically acquainted.

Another and a prominent source of difficulty in discriminating affections of the skin may be looked for, either in the inability of the patient to give, frequently from his ignorance, but oftener from the obscurity of its origin, and the length of time elapsed previous to its demanding his serious attention, or a deficiency of tact in his medical adviser, to elicit an accurate history of his complaint.

This last source of error is of more frequent occurrence, and of greater magnitude than may be readily supposed; and the importance of keeping it in mind, and if possible of avoiding it is forcibly exemplified in the events of the case which I have made the subject of this article. Although, it is probable, all the grounds of mistake above mentioned were in existence; yet to the absence of a minute inquiry into the history of this disease may be attributed the mistaking of an affection innocuous in its character for several of the most cruel complaints to which "flesh is heir;" and, as a consequence, the subjecting

of the unfortunate sufferer to restraints and remedies as severe as unavailing. To the injurious operation of this cause of misconception, I must, in common with others of my professional brethren, subscribe; for previous to obtaining a detail of its origin and course, I was deceived as to its class among complaints, and sought in various accounts of lepreasis, vitiligo and elephantiasis for a description of, and a remedy for, the disease.

The subject of this case is a female slave, (belonging to Mr. J. P.) a native of the country, of what is called a brownish black complexion, possessing a firm habit, good general health, and about the middle of life. The affection of her skin which has attracted towards her the notice of medical men is in appearance of a tuberculous character—consisting of numerous elevations of the uniform height of about two lines, and varying from half an inch to eighteen, twenty-four, and thirty inches in circumference: hence covering the greater part of the body, and giving to her whole person an aspect as rough as that of the rhinoceros. These elevations are no where confluent, and have never ulcerated. The smaller ones are regular in their outlines—hemispherical, oblong or oval; the larger are irregular in shape, with rugose edges somewhat more elevated than their surfaces: which in both kinds are smooth but not glossy. Their colour is the same, their texture appears the same, and their sensibility is nearly as acute as that of the unaffected skin. Every part of her surface, except the face, palms of the hands, and the callous parts of the soles of the feet, seems equally subject to their invasion.

In the account which the woman upon minute inquiry gives of her disease she states, that the earliest notice she had of these tubercles was at puberty; that they always *originated in a rupture of the skin*, frequently of the slightest kind—as the prick of a pin, the sting of a hornet, the scratch of a thorn or a trifling cut or burn; that some of them have continued to increase in size for years after the accident producing them, while others have attained their acme in a few months or weeks. She likewise says, as intimated above, that they have no effect on her constitution; and indeed, that they produce no other inconve-

nience than a stiffness of motion, and particularly of those limbs whose joints are covered by them.

The most unpleasant circumstance, at least to the woman, attending this unsightly complaint, arises from its being hereditary: her children being affected as they arrive at puberty in a similar manner, though not as yet to so great a degree, from the same causes.

This is the most accurate description, that the abscence of intelligence in my patient allows me to give, of the affection, which, in contradistinction to the opinions of one or two other physicians, I venture to call an exuberant extension of the healing process by which a cicatrix is formed. The grounds upon which I give this diagnosis are, its obvious dissimilitude to the relations furnished us of the complaints for which it has been mistaken; its always originating in a wound; and because large cicatrices similar in appearance to those above described, from small injuries (though seldom of so great a size as to lead to mistake of character) are common to all the varieties of the human family, and are among negroes of very frequent occurrence.

There is here an evident disposition in the system to extend the cutaneous structure beyond the healthy limits assigned to that organ, and it cannot be considered wholly uninteresting to attempt to inquire into the peculiar diathesis causing so great a violation of the usual order of organization. Indulgence in this endeavour may be the more readily granted, as the state of the system accompanying as well as the manner in which it produces this complaint, can, I conceive, be so readily understood that but a moment need be occupied it its examination.

An essential property of every disease is a change of action in the secernents of the part affected, by which the fluids they secrete in the healthy state are altered in quality, diminished or increased in quantity, or wholly stopped; and these changes are different in different temperaments. Upon this altered agency of the capillaries depends the growth of tumours, tubercles, and all other morbid enlargements of structure; and, in like manner, the power by which wounds are healed. The individual

under consideration possesses, manifestly, an excess of fibrine in her circulation, with a more than common proclivity to irritation of the surface from slight causes. In such a state of the system, besides the usual process, which, by the effusion of coagulable lymph, and the formation of new vessels, is instituted to unite the separated organization of wounds, the law of the animal economy above mentioned, acting with morbid energy, would seem to impart to the new secernents the power of secreting more lymph, and forming other secernents: the action of which power would be limited, only, by the inability of the vessels primarily divided to trasmit them their nourishment.

Why this disease did not present itself till puberty may be explained by the facts of the little fibrine anterior to, and its great increase in the blood-vessels at this period of life.

With regard to the treatment I must observe that the arsenical solution, mercury and other constitutional remedies have had, what indeed must be obvious, no effect upon the disease, while, with the exception of the bandage, which has seemed to check its growth, topical applications and particularly escharotics have tended to increase it. It may, however, be proper to mention that a respectable old practitioner assures me he has succeeded in removing similar affections of the skin by the repeated applications of a blistering plaister.

ART. V.—*An Essay on Fever, read before the Medical Society of Lexington, and Published by their request.* By JOHN E. COOKE, M. D. &c.

ALTHOUGH the mode in which the remote causes of fever produce the symptoms constituting that disease has been discussed for ages, the subject still engages the minds of the inquisitive. But notwithstanding there are still important points at issue, there are some that have been settled; and these fixed points are like rocks on which we may build with confidence.

Thus, we consider as a settled point that cold, the unknown agent commonly called miasmata, a violent concussion of the body, &c., are capable of producing fever. We consider it settled also, that in the progress of the patient, exposed to the operation of these causes, to fever, the action of the heart is debilitated; that a state of congestion in the interior vessels takes place; and that increased action of the heart follows these. Whatever other phenomena occur, these invariably appear. In observing the whole course of a tertian fever, we perceive that the system periodically returns to a certain state, having passed through the series of changes above mentioned. which regularly recur in the same order during every period. This has been observed from time immemorial, as we discover from perusing the medical records of every age and nation. It is therefore a settled law of the human system, that when any cause produces the first of this series of states, it must run through the whole series if the depression be not so great that the system is incapable of it.

There are two states of the system, the extremes of this series, which are as opposite as can be supposed to exist; and they have been designated by various authors by terms indicative of this. Such are the terms apyrexia and pyrexia, cold stage and hot stage; decreased and increased action; collapse and excitement. All these terms, invented or applied by different observers

at different periods, show that all have assented to the fact, that the system is at first in a state of diminished action and afterwards in a state of increased action, in every paroxysm of fever.

In endeavouring to form a theory of fever, an eminent physician, Dr. Cullen, took it for granted that the several states through which the system passes in a paroxysm of fever are in the relation of cause and effect to one another; each producing that which succeeds it. The circumstance of their invariably following, in the same order, the operation of the remote causes of fever, is strong ground for this presumption. Those things which invariably occur we must consider as necessary to the final result: and as the order is as invariable as the occurrence of the states themselves, the order in which they occur, is as indispensible as their occurrence. A state of diminished action of the heart is therefore necessary to the existence of the increased action of fever. That which is necessary to the existence of a state of the system; without which that state cannot exist; and which whenever produced is followed by that state, if the system is capable of it, there is strong ground to believe, produces it. We should not however be satisfied with so general a conclusion, as that the state of diminished action in some unknown manner produces the state of increased action. We should endeavour to ascertain with certainty whether these states produce, each that which follows it: and if possible, in what manner.

We may in pursuing the inquiry investigate the operation of the remote causes in producing the first morbid impression, and afterwards the effect of this impression on the system, and the effect of that effect, and so on: or we may inquire into the cause of the last observed state of the system, and then into the cause of that cause, and so on, until the investigation results in the discovery of the whole connexion between the remote causes and the symptoms observed in fever. We propose to adopt on the present occasion the latter mode.*

The series of cause and effect operating in this case consists

*The former is followed in the investigation of this subject in a former number of this Journal.

of the following, as we have already seen; 1, the remote causes; 2, a state of debility and we would particularly note, debilitated action of the heart; 3, a state of congestion in the internal vessels; 4, increased action or febrile action.

In pursuance of the course proposed, we first inquire into the cause which produces the increased action of the heart observed in a febrile paroxysm.

The heart is stimulated to action by the blood which it circulates through the system. The following passage from Bell's anatomy places this in a clear and strong light. He says, "When we tie the two venæ cavæ so as to prevent the blood from arriving at the heart, the heart stops; when we slacken our ligatures, and let in the blood, it moves again; when we tie the aorta, the left ventricle being full of blood will continue struggling, bending, turning up its apex, and contracting incessantly and strongly, and will continue this struggle long after the other parts have lost their powers."* Here we find that when the entrance of the blood into the heart is prevented, the heart ceases from acting; when the blood is let into it again, it acts again; and when by tying the aorta, the blood is prevented from escaping from the heart, its continued presence produces incessant action of that viscus. The blood is therefore that which stimulates the heart to action: and as withdrawing the blood stops the action of the heart, letting it into the heart excites it to action, retaining the blood in the heart produces incessant action, increasing the quantity poured into the heart must increase its action, provided some cause do not interfere to prevent this effect.

An increased quantity of the blood pressing into the heart must therefore produce increased action of that viscus, if some cause do not interfere to prevent it.

During the increased action of fever there is such an increased quantity of blood pressing into the heart, and therefore the cause shown to be capable of producing the increased action is actually present at the time the latter is observed. An in-

* Bell's Anatomy, vol. 1, p. 342.

creased quantity of blood pressing into the heart may proceed from an increased rapidity of circulation, or from an excessive accumulation of blood in those vessels which immediately supply the heart with that fluid, viz. the venæ cavæ. In this case, however, diminished action of the heart precedes the increased action. There is both diminished rapidity of the course of the blood and diminished size of the pulse, so that a lessened volume of blood has been for one or more hours pouring into the heart, when the increased action commences. At the same time there is at the very commencement of the increased action, such a congestion or accumulation of blood in the venæ cavæ as necessarily to produce a pressing of the blood into the heart. There is therefore in the state of congestion a sufficient cause, in the increased quantity of blood pressing into the heart, for the increased action observed to follow such a state, whenever the heart is able to act with vigour.

We next inquire, what is the cause of this state of congestion of blood in the interior veins? Congestion consists in an accumulation of blood in the vena cava and the great branches entering into it from the abdominal viscera, and the internal and external parts of the head. The veins are passive in this case. They have no power to retain the blood in their cavity. All the power they have is to carry on that fluid. The heart is the prime mover of the circulating fluid. When it acts strongly, it sends on the blood in a full stream; and when its action languishes, the stream of blood cannot fail to correspond. Diminished action of the heart therefore must necessarily be the cause of a diminished stream of blood into the aorta; and consequently of an increased quantity left behind in the vena cava; that is to say, it must produce congestion in the internal veins.

This congestion has however been attributed to a contraction of the capillaries. Let us see whether such a contraction can produce congestion. The certain consequence of such a state of the capillary vessels, it is true, would be to propel the blood in them into the veins. But what is to retain it there? It is evident, as has before been observed, that they have no power to restrain its course and cause the accumulation we find in such

cases at the right side of the heart. Unless the action of the heart be diminished the blood must be drawn by its dilatation from the veins, and be propelled into the aorta and its numerous branches: and in them it must accumulate, if the capillaries be in a state of constriction. But we find no such thing on examining the bodies of those who die in this state. We find no blood in the arteries—we find it all in the veins.

When a circulating fluid is obstructed in its course, it must accumulate immediately behind the obstruction, and press upon it so as sometimes even to endanger the vessels containing it. In the case now under consideration, the accumulation is invariably behind, that is, at the right side of the heart, and in such quantity as frequently to burst the vessels entering into the heart. There is no accumulation behind the capillaries. This obstruction must therefore be in the heart, and not in the capillaries: and this obstruction can only arise in these cases from diminished action of the heart, which at the time actually exists.

We have now seen that of this series of states of the system, each flows out of that which precedes it; there being in each something which, in a very simple and natural manner, produces that which follows it. The next inquiry is, do the remote causes all produce weakened action of the heart? That they all do is certain, because this state of the action of the heart is the invariable precursor of febrile action, so as to constitute a part of what is called by medical writers a paroxysm of fever. It is therefore a part of that effect produced by the remote causes; and consequently all the remote causes of fever produce either directly or indirectly, weakened action of the heart. The fact that they do so is also ascertained by observation of the operation of the several causes. Thus, a blow or a fall of some violence will instantly reduce the action of the heart so low that it can scarce be felt; or even extinguish it entirely. Cold reduces the action of the heart rapidly and if continued, at a very low degree of temperature, extinguishes it completely. Some have asserted that cold excites the action of the heart. This is not true. There can no instance be produced, in which *during continued exposure to very cold water*, the action of the

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did not actually sink and continue to do so without ever rising to that grade of action that it had immediately before the exposure. It is only when a person is exposed to cold, as by immersion in cold water for a short time, and is then withdrawn from it, that the action of the heart increases. The increase of action therefore is the consequence of withdrawing the cold, not of applying it: and the reason of this increase is obvious from what has been just established. The exposure to the cold weakens the action of the heart; this produces an accumulation of blood in the vena cava and its branches, on the right side of the heart; and this increased quantity of the natural stimulus of the heart, as soon as the depressing power, cold, is removed, excites that viscus to increased action.

Thus far we are on sure ground. But the insatiable curiosity of man impels him to inquire still farther. How is this effected by the remote causes of fever? There is a limit beyond which finite powers cannot penetrate—there are depths which they cannot fathom. We know the fact that a certain degree of temperature produces the evolution of the embryo members of the chick in the egg. How, we know not. We may conjecture—and when we have wearied ourselves with conjecture, and ask are we right, all the answer we can receive from reason, is, *it may be so.* The limits of our powers of investigation, however, are only discovered by experience, in the course of exercising them; and we often succeed, after repeated failures, in tracing the connexion or relation between different things which at our first attempts seemed inscrutable. We should therefore not be deterred from attempting the solution of difficult questions; but we ought to take care to avoid error, in proportion to the difficulty and obscurity that surrounds us. To proceed then in our investigation: A severe blow will in an instant reduce a man in perfect health to a state of debility and languor precisely like that observed in the cold fit of an intermittent. He will sometimes lie for a long time with a feeble pulse and pale countenance. Other remote causes do not produce these effects until after exposure for some time; for days, or even weeks. How it is that a blow produces this effect, I confess

I am unable to say. It may be alleged that it is by influencing the brain and nervous system. But a blow on the back, the stomach, or the breast particularly near the heart, produces the same effect as on the head: even a severe blow on the limbs produces it. Moreover the action of the heart is, by a very violent blow, arrested instantaneously—There is not the slightest difference in time between the cessation of the action of the heart and the cessation of nervous influence. And as we know that the dependence of the heart and arteries and the nerves is mutual; and also that the loss of the supply of the arterial fluid is as destructive of the power and action of a part as the loss of the supply of nervous influence, who shall say which is first affected and which secondarily? Who shall say whether the heart is affected through the medium of the nerves, or the nerves through the medium of the heart and arteries?

Cold also weakens the action of the heart. We know that cold air has access to the lungs as well as to the external surface; but as when a man is exposed to a very low temperature in cold water, it is the surface only that is acted on, we shall consider what influence cold can exert upon the system when applied to the surface only.

If we were to consider the body as a mere mass, without a circulating fluid, the temperature of the whole must be reduced by the application of cold water to its surface; because heat passes through the whole without any hindrance, so that a diminution of the temperature of the external parts must result in a diminution of that of the internal. But when from the interior of the body a fluid, the blood, is perpetually passing to the external parts and back again, the temperature of this fluid must be lessened while passing through those parts immediately exposed to the cold applications. This is confirmed by the observation of Currie in his experiments on the effects of immersion in cold water. He says, that the temperature of the inner parts of the body, (ascertained by placing the bulb of the thermometer in the mouth of the patient which was closed round it as completely as possible) fell eleven degrees below what it was before immersion. A gentleman who graduated

in this school two years ago performed an experiment to ascertain the effect of immersion in cold water on the temperature of the blood. He found that a current of blood flowing from the vessel of a dog which was immersed in cold water, was several degrees colder than a stream from the same vessel before the immersion. Here is evidence, then, that exposure of the surface to severe cold reduces the temperature of the internal parts and of the blood. It is indeed not necessary to resort to experiment to show that cold acts upon the whole system although the surface only be exposed. A certain temperature being necessary to the performance of the functions of life, there is a provision made in the system for the continual evolution of heat, which passing throughout the system escapes from the body in such quantity as to be sensible. The rapidity with which it escapes depends upon the temperature of the medium in which the body is immersed, viz. the air or the water. Lower the temperature of this considerably and it passes out so rapidly that the temperature of the body falls, Currie says, 11° in water at the temperature of 40° in a few minutes. On the contrary raise the temperature of the surrounding atmosphere to that of the body, and the passage of heat from the body in the ordinary way is checked, and it must accumulate in it, unless there be a provision made for its escape in some other way, as there is. It is evident therefore that cold or warm applications to the surface affect the temperature of the whole body. We know also that a certain degree of heat is necessary to the due performance of the functions of life; just as a certain degree is found necessary to enable the system of the chick in the egg to perform its functions. Without that degree it will remain quiescent for ever; with that degree the evolution of all its powers will commence and go forward. In the progress of the evolution, lessening the temperature will arrest the progress, and unless it be speedily restored, for ever. John Bell says (1. 339), speaking I believe of the chick in the egg, "The heart feels when it can be called nothing more "than a punctum saliens, the slightest change of heat, or cold." This is perhaps the nearest approach that we can make to the explanation of the operation of cold in

reducing the action of the heart. How it does so, we can no more say than we can tell how sugar produces the sensation which we call sweetness when applied to the tongue.

Miasmata constitute another remote cause of fever weakening the action of the heart. In what manner, is the question.

We observe in all those who are exposed to the operation of this cause, that the blood is darker than usual: in some it is nearly black. It is not said that the arterial blood is so dark; but the blood drawn from the veins of the arm. It is however evident that as this blood is so much darker than usual, the arterial must likewise be of a darker colour; particularly as the blood from the veins of the arm is so near the arteries and has so lately passed from them. There is however reason to believe that the arterial blood itself is sometimes of a very dark hue. I saw a man in a sickly autumnal season, who lived in a place much exposed to miasmata, and was attacked by hæmoptysis, discharge from his lungs, by coughing and spitting, blood so black that with difficulty I could believe that it came from that part, and was not convinced until I saw him cough and spit it out. This dark-coloured blood, it is known, is less stimulant to the heart than the florid blood. The experiments of Goodwin have long since established this point: viz. that whenever the blood passes to the left side of the heart, of a dark colour, the action of the heart immediately declines, and revives as soon as the blood changes to the florid colour again. This continually occurred in every experiment. Here we have therefore a reason for the weakened action of the heart in the dark colour of the blood observed in those who are under the influence of miasmata.

But how do miasmata prevent the change in the colour of the blood necessary to render it sufficiently stimulating? Here again, I must acknowledge I know not. The fact however is most certain, that blood of a dark colour is not capable of stimulating the heart to the strong effort that florid red blood is.

We might run round the whole list, and in perhaps every instance be under the necessity of admitting, that the manner in which the remote causes produce weakened action of the heart

is to us unknown. Indeed we may go so far as to say that the manner in which the remote causes act in producing their first impression, their direct effect, cannot be discovered. This we can never know how heat and light excite; or how cold and darkness depress. The fact however is clear that these remote causes do in some way produce weakened action of the heart; it is equally certain that weakened action of the heart must suffer the blood to accumulate in the vena cava and its branches; and this accumulated blood, pressing for entrance into the heart, must excite it to increased action, as soon as, by the removal of the depressing power that may be operating on it, it can act freely; or when the quantity of blood presented to the heart is suddenly increased, as during the convulsive motions of the body in an ague, even though the remote cause be not removed.

It will be observed that in this explanation of the operation of the remote causes in producing fever, a cause capable of producing the effect observed in each step is assigned, and that cause is shown to be present at the time the effect is produced. Thus it is shown that the blood is the natural stimulus of the heart to action, and that when an increased quantity of this stimulus is suddenly presented to the heart, it must produce increased action; and it is shown that this cause is actually present at the time that increased action in fever comes on.

It is shown that congestion or accumulation necessarily follows weakened action of the heart; and that this cause of congestion, weakened action of the heart, is actually present at the time in which congestion takes place.

It is shown that weakened action of the heart is produced by the operation of certain agents, and in every case of fever, the history of which can be traced, the patient has been exposed to the operation of one or more of these causes.

Finally, it is shown that whenever these agents produce weakened action of the heart, this congestion and increased action follow, provided the heart be not prostrated so low as to be incapable of acting.

It has been asked, cannot the heart be excited to increased action by other stimuli than an increased quantity of blood? For

instance by heat? It is granted that an increase of temperature does produce increase of the action of the heart. It does not, however, produce the increased action of fever. That action which it does produce is temporary and declines when the temperature is lowered. It cannot produce the increased action of fever, *directly*. Before this can take place the excitement produced by high temperature must decline and weakened action take place; and then increased action will follow although the temperature has been reduced. High temperature therefore produces the increased action of fever only through the medium of the same state which is produced likewise by low temperature, viz. weakened action of the heart. It is not therefore an elevated temperature which produces the increased action of fever. This is often absent when fever is produced; whereas the cause assigned, accumulation of blood in the vena cava and its branches, is always present when the increased action of fever is produced. It will be observed that it is not denied that heat will increase the febrile action, when present: heat will undoubtedly aid in increasing the effect of the increased quantity of blood presented to the heart; although it cannot, as has been before observed, produce the increased action of fever without first producing weakened action and consequent accumulation. Whereas increased action will take place in a very high degree even in a low temperature, as in winter, when accumulation of blood in the vena cava has been suddenly produced by exposure to severe cold. Accumulation of blood in the vena cava and its branches therefore is the sine qua non, the indispensable prerequisite to the increased action of fever; the cause which produces it where elevated temperature is not present; which produces it for instance, in a man who is immersed for half an hour or a less time in cold water, and is then laid on a bed in a cold room; which produces it where no conceivable stimulus is present, but that accumulation which is both present and capable of producing the increased action which follows.

We have now seen that three states of the system exist in every fever; viz. weakened action of the heart, accumulation of blood in the vena cava and its branches, and increased action

of the heart; each producing that which follows it. These three states of the system produce all the symptoms observed in a paroxysm of fever.

Weakened action of the heart of course is the cause of the diminished force with which the blood enters the arteries; that is, is the cause of the weak pulse. It is also the cause of the diminished quantity of blood in the arteries. Hence diminished bulk of the external parts of the body which in a great measure consist of the fluids in the vessels. The blood not being sent in the usual quantity into the capillary vessels, they contract, as all vessels do in proportion to the reduction of their contents. If this were not so; if the vessels did not contract when the fluids in them are diminished, there would be a vacuum, which never takes place in the system; in the soft parts at least. The skin itself contracts after having been distended, as soon as the distending fluid is removed; as when dropsical swellings are reduced, by remedies or by tapping. We find then, that the constriction of the capillaries is the consequence of the diminished quantity of fluid sent to them, which is the consequence of weakened action of the heart.

The surface owes its colour to the blood circulating in the vessels of the skin. The quantity of blood being greatly diminished the countenance becomes pale.

Weakened action of the heart, therefore, produces the symptoms, weak pulse, diminished bulk of the external parts, and paleness of the countenance.

Accumulation of blood in the vena cava and its branches affects all those parts in which these vessels are seated.

It must distend and enlarge these vessels and consequently enlarge the parts chiefly constituted by them and their contents. The liver, which is made up almost entirely of veins, as well as the spleen which is little inferior in this respect to the liver, should be greatly enlarged by accumulation of blood in the vena cava and its branches. This we find to be the case. No parts of the system, are so variable in size; and they are remarkably enlarged when accumulation of blood in the vena cava and its branches takes place.

The vena cava is also considerably, sometimes greatly enlarged. In a healthy state there is a dilation and contraction of that part of the vena cava near the heart, which necessarily arises from the contraction and dilation of the heart. During the contraction of the heart, the ventricle being closed, the current of blood from the vena cava is arrested, and consequently accumulates in it during the contraction of the heart. During the dilation of the heart, the current flows into the ventricle and the distension of the vena cava is taken off. This contraction and dilation, Bell says he has certainly seen.

This dilation of the vena cava, when that vessel is greatly distended by unusual accumulation of blood in it, must be more remarkable, and may extend to such a distance as to be perceptible in the abdomen.

There is such a pulsation frequently observed in the abdomen on the right side: it occurs when the pulse is weak, and is greatest when it is weakest; and even continues, at least in some cases, after the pulse can no longer be felt.

This pulsation has been mistaken for an aneurism of the aorta. This however it cannot be, because it is on the right side; because it is most perceptible when the pulse is weakest, disappears when the pulse is raised, and appears again when the pulse falls low; and because it can often be cured by such remedies as lessen the accumulation of blood in the vena cava.

It may be remarked that the pulsation of the vena cava corresponds with that in the arteries, as it must do: for, the dilation being caused by the arrest of the course of the blood into the heart during its contraction, occurs at the same moment in which the blood driven into the arteries distends them, also.

The same accumulation produces also derangement of the secretion of those glands in which the branches of the vena cava are seated. Secretion is effected by the bloodvessels; and the vessels in each gland require for the due performance of their function, a state adapted to the work each has to do. If this were not the case, we should not find so great a variety in the manner in which the glands are constructed and in which they are supplied with materials to work upon. Accumulation

of blood in the veins of these glands, sometimes to the degree of rendering them twice, thrice, or five times as large as in the natural state, must derange their functions. Accordingly we see, when congestion exists, that the secretions of the liver, the kidneys, the stomach, and the bowels are more or less affected.

The vessels which enter a gland carrying into it blood for the purpose of secretion, are minutely subdivided in the substance of the gland. The blood left after the object is effected, is returned by minute veins, which receive it from those vessels which carried it in. The secreted fluid is also carried by minute vessels into a common duct or ducts. It is obvious therefore that the veins and the minute ducts receive the whole of the fluid carried in by the vessels. These vessels therefore may be considered as branching in the interior of the gland, and each minute branch terminating in a returning vein and a duct. In this state of things an increase of the fluid flowing through the gland must increase the quantity of fluid secreted, unless some obstruction arise. Such an obstruction does arise, as we observe in other cases, from an excessive quantity of fluid in the vessels. When a gland becomes hard by distention, secretion is arrested or ceases. According therefore to the degree of accumulation, different effects are produced on the glands affected by it. Moderate accumulation produces increase of secretion—excessive accumulation results in its suppression.

An increase of biliary secretion will produce nausea, vomiting, purging: a decrease of the same occasions a constipated state. Sometimes the secretion is so deranged that the fluid secreted is not bile, but a dark or green fluid without any of the properties of bile. In some cases the fluid passed from the liver is pure black blood; the evidence of which is, that the discharge changes insensibly from black blood to black bile, or from the latter to the former; so that it is impossible to say at all times whether the fluid discharged is blood or bile. The blood discharged thus into the bowels from the liver, is sometimes rejected by the stomach, sometimes passes downwards.

Other glands are likewise deranged in their secretion. Thus

the secretion of the stomach is sometimes increased and sometimes diminished; whence in accumulation of blood in the vena cava and its branches, there is sometimes excessive appetite and digestion, and sometimes both are defective. In consequence of a defect of secretion of the gastric fluid, imperfect solution of the food is observed; whence arises fermentation of many things eaten in this state, and consequently acidity and flatulence in the stomach.

So the secretion of the kidneys is in the same state, sometimes increased, sometimes diminished.

Pain also, in various parts, in the liver, the spleen, the stomach, bowels, kidneys, or the head, is produced by distention of the veins, branches of the vena cava, situated in them. That pain is produced by great distention of the blood-vessels, is certain; and when we see pain occur during the accumulation of blood in the vena cava and its branches, and increase as the pulse becomes weaker, and disappear on increasing the pulse, we can but conclude that distention of the veins causes the pain.

Thus we find that some of the leading symptoms in fever, are produced by accumulation of blood in the vena cava and its branches. Many others might be noted; but for this we have not now sufficient time.

The confirmation of this doctrine is, that evacuations from the vena cava and its branches, through the liver, relieve the symptoms mentioned more speedily and more effectually than any other plan of treatment.

This doctrine is strongly confirmed by other circumstances. The symptoms above mentioned, excepting increased action of the heart, constitute most of the important chronic affections. They occur in the access of fever; and after the subsidence of febrile action, if the proper means have not been used to remove the state which produces them, they show themselves in the form of some chronic disease, as a sequel to the fever. Moreover, although some one part of the system be in general more strikingly affected than others, the other parts connected with it through the medium of the vena cava and its branches, are also more or less affected. Thus if the disease be called

dyspepsia, in consequence of the stomach being most remarkably affected, yet will also the liver, kidneys, and head be disordered more or less. So if the head be chiefly affected, so that the disease be called vertigo, coma, &c. yet will the stomach, liver, and kidneys be disordered, and so of the rest: in all these diseases palpitations and enlargement of the vena cava will frequently be observed; at least in extreme cases.

From this statement it would also follow that the same mode should be adopted in the treatment of deranged secretion of the liver, as in jaundice, constipation, bilious diarrhœa with scanty discharges; in the treatment of deranged secretion of the gastric juice, as in dyspepsia and excessive appetite; of the kidneys, as in nephritic affections, and what people commonly call gravel; of the head, as in vertigo, &c.; of palpitations, and of some others not mentioned; and that the mode of treatment should be essentially by evacuations from the vena cava and its branches through the liver.

The whole of this is a fair inference from the premises; and the success attending the practice confirms in the best of all ways the truth of the doctrine.*

Having pointed out briefly the effects flowing from the two first states of the system, the state of debility and chiefly of weakened action of the heart, and the state of congestion resulting from it, it remains only to mention the effects produced by the third, increased action of the heart. This obviously fills the arteries, and consequently expands the features, restores the bulk of other external parts, and communicates to them an increase of temperature and of colour; all which symptoms are observed in the hot stage of a fever.

The excessive action of the heart moreover, is so exhausting that it is rarely maintained twelve hours before it declines; and it commonly falls below its ordinary grade, sometimes into extreme weakness. The inevitable consequence of this is a return of the accumulation of blood in the vena cava and its branches and with it a repetition of the whole paroxysm.

* These statements are necessarily condensed in this paper: They are to be seen at large in a treatise of Pathology and Therapeutics published by the writer.

ART. VI.—*On the Sanative Influence of a Southern residence on Northern patients.* By HENRY J. PECK, M. D. of Catahoula Parish, Louisiana.

BEFORE we commence an investigation into the effects that are wrought on the human system, by a change of residence from the north to a more southern latitude, we shall proceed to mention a few of the distinguishing characteristics of a southern climate. Our observations on the subject were made in Louisiana and Mississippi. Hence when in the course of our remarks, we allude to a southern climate, we mean no other than that of the states above mentioned. The climate here differs from that of other states more remote from the equator, in two respects, viz. in the degree and duration of heat: for while the summer, in this southern country, is several degrees warmer, it also continues a month or two longer than the same season in the northern or middle states. There is also a still more striking difference. We allude to the difference in the temperature of the nights: for while they are comparatively very warm in the middle states, they are quite cold at the south. This would appear to involve a contradiction, upon a superficial view of the subject; but when we investigate the matter philosophically, the difficulty at once vanishes.

Evaporation depends on the degree of heat, and in all cases it must be greatest where the degree of heat is most intense, provided there is a full sufficiency of moisture in both cases. Hence it must be acknowledged that in Mississippi and Louisiana, where moisture abounds, that the evaporation must be greater than it is in Kentucky, or other states in the same parallel of latitude. This must continue to be the case through the day, so long as the rays of the sun are sufficiently vertical. But when the sun sets, the caloric that rarified the moisture and thus raised it from the earth, and rendered it specifically lighter than the atmosphere beneath, is withdrawn, the moisture

condenses, and falls in the form of dew. The consequence that ensues on the falling of a very heavy cold dew is very manifest; for in passing through the subjacent air, it absorbs a considerable portion of its heat. Independent of this, the earth during the day accumulates a considerable quantity of heat, which continues to combine with aqueous particles, and to arise in the form of vapour some time after the going down of the sun. This ascends but a small distance from the earth before it loses a portion of its heat by coming in contact with cool air: thus having lost a considerable part of its heat, it necessarily becomes specifically heavier and falls to the earth. By this process the nights are rendered quite cool. Having thus mentioned a few of the differences of climate, so far as relate to heat and cold, we shall without further prelude proceed to the consideration of our subject.

The immediate effect produced on the human system, by the direct application of heat to its surface, is to increase the secretions from the skin, which are in proportion to the degree of heat applied. Hence so soon as the sun is sufficiently elevated above the horizon to have communicated its heat to the atmosphere, the skin pours forth its sensible perspiration. This usually commences about eight o'clock in the morning, nor does it cease its operation until late in the evening. But while the secretions of the skin are thus poured forth by the direct application of heat to the surface, there are other glands whose secretions are elicited in an indirect manner. These are from those organs which contribute to form the digestive apparatus, but more particularly the liver, whose action is excited by its intimate association with the skin; or, in the language of the schools, by cutaneo-hepatic sympathy. The hepatic secretion is very much increased in *quantity*. On this part of our subject we will remark that we believe the action of this viscus bears some relationship to that of the skin. This we think is the fact in most cases. Whether there is an exact ratio in the quantity excreted by the two glands, we do not pretend to determine, and we leave this matter to those who are a little more fond of hypothesis and vague conjecture, than ourselves. This much,

however, we vouch for, that a much larger quantity of bile is secreted in this climate, under similar circumstances, than in regions farther north. The healthy biliary secretions in this climate differ very much in *complexion* from healthy hepatic secretions of a country farther north: for instead of presenting a bright rhubarb colour, they have a dark brown appearance, even in the winter season, but more particularly in the summer months. There is such a striking difference in this respect, that one unacquainted with the fact, would unquestionably suppose the existence of morbid action in the liver, provided he formed his opinion from this symptom only. The rationale of this phenomenon we presume may be referred to the atmosphere that is inhaled into the lungs of the patient. For as a cubic foot of rarified air, can not contain as much oxygen as the same quantity of condensed or cool air, it must necessarily follow, that the same patient cannot inhale as much oxygen, at one inspiration in a southern climate, as he could farther from the equator, at the same season of the year and under similar circumstances. Consequently less carbon will be eliminated from his lungs; and hence it follows that his blood can not be so well arterialized or oxygenated. When this blood is returned to the left ventricle of the heart and is again injected into the general circulation, it must be much darker than it would otherwise be: and when such blood in the course of the circulation comes within the province of the liver, and is operated upon by that organ, it must necessarily produce bile of a much darker complexion than it would otherwise possess. This bile when excreted freely into the alimentary canal, must necessarily cause the excrement to be of a dark appearance.

There is not that degree of *consistence* in the alvine excretions here which obtains to the northward. This we presume is brought about by an increase of the serous secretions from the intestines generally during the night commingling with those from the liver, and excrementitious substances. We mentioned in the prelude to this subject, that the nights were quite cool: now the effect which this abstraction of heat must immediately produce upon the surface of the body, is at once to stop the se-

cretions from the skin, and indirectly by sympathy to check the secretions from the liver. The same cause which thus partially suspends the action of the skin and liver during the night, increases the secretions from the bowels by producing a determination of the circulation to these organs. But we may be asked upon what data we ground this assertion. We reply that it is admitted by all that cold applied to the surface of the body does not increase hepatic secretion: for if the reverse were the fact the liver would secrete more freely during the winter than in the summer months; and, furthermore, diseases of the liver would abound in northern countries, and diseases of the pulmonary organs would abound in southern countries. But this is not the case. We are conscious however that cold applied to the walls of the abdomen will frequently succeed in bringing about alvine evacuations, when cathartics and enemata have been unsuccessfully administered. Hence we infer that watery secretions from the small and larger bowels are increased during the night. This continues to be the state of things until a determination to the surface on the ensuing morning takes place. Thus we have shown that the alvine evacuations present three distinguished characteristics in this country; viz. increased quantity and colour, and diminished consistence.

These effects wrought on the human system by a southern residence, must according to the letter of our text, have a sanative influence on northern patients: particularly those afflicted with dyspepsia, chronic, habitual constipation, and chronic diseases of the pulmonary organs generally. This class of diseases, and a vast variety of others whose pathology consists in morbid congestions and sub-acute inflammation of the thoracic and abdominal viscera and their appendages, are thus frequently relieved without the aid of medicine. And in what manner? Merely by copious secretion, more particularly from the liver. Here we may read an important lesson from the book of nature, in the words of a British poet,

> "First follow nature and your judgment frame
> From her just standard which is still the same."

So soon as the changes which we have already mentioned, begin

to develope themselves in the system of the dyspeptic, his symptoms gradually improve; he finds that he is not compelled to take so much medicine to elicit an evacuation from his bowels; he begins to learn that by degrees he can eat vegetables and fruits of different kinds; he sleeps more profoundly at night; he is not so frequently distressed with sour stomach, and is not compelled to lay in a dormant posture after his meals: gradually all his symptoms vanish away, and the organs of his digestive apparatus are once more restored to their wonted energy, and with new life perform their healthy functions. Habitual chronic constipation is in most instances cured.

From the views which we have taken of the subject now under consideration, it will be entirely superfluous to go further into detail in relation to the manner in which this disease is relieved, in as much as we have stated, somewhat at large, the effects of the climate on the human system, and its modus operandi in relieving the abdominal diseases of northern patients.

Pulmonary affections are relieved very much in the same manner. For when we reflect that the air which is inhaled into the lungs is less stimulating than it is farther north; and furthermore that free secretion is established in the skin, and that a concomitant degree of action is established in the liver, it is not surprising that the congested state of the lungs, or their appendages, should be relieved by an increased determination of the circulation to other organs, and increased action in the absorbent system, which is brought about in an indirect manner. But it is not our intention at present to enter into detail and follow up in physiological manner the progress of cures which so frequently occur here. Our remarks for the present are intended as a beacon, to attract the attention of the profession to a source of relief that is too often neglected. We are well aware that the changes which occur here in the animal economy, are referred by northern physicians to the influence of the Mississippi water: but this we are well satisfied is erroneous. In fact most patients who have been most perfectly relieved by a residence in the country (so far as our observation has extended) have resided at a distance from the river, and have not drunk

of this far famed water. If this were the main agent in relieving the dyspeptic, he might be relieved with as much facility by drinking a few bowls of it in Canada as in Louisiana. But this is not the fact, and we must look elsewhere for the causes which are instrumental in revolutionizing the system and in eradicating disease. We look upon heat, continued for a considerable time, as fully sufficient to effect all those changes which occur in the systems of those who are not acclimated; and if any other agent is instrumental we confess its nature is so subtle that our senses are not sufficiently acute to take cognizance of it.

ART. VII.—*Case of Epilepsy resulting in Insanity, cured by continued moderate purging.* By WILLIAM W. WORTHINGTON M. D. of Washington County, Mississippi.

[*Communicated in a letter to Professor Cooke.*]

ABOUT the first of July last, I was called to see a Mr. Black of this county, aged sixty or sixty-five, who had been severely afflicted for the last ten years of his life with epileptic convulsions. They at first made their appearance at long intervals; but as age advanced and the system became more enfeebled, the intervals grew shorter until he had one or two every day. He was incapable of much exercise, and the slightest fatigue never failed to bring on convulsions. He is a blacksmith by trade, and supported a large family for many years by the use of the hammer; but was compelled, long since, to abandon his trade in consequence of his entire inability to undergo the fatigue necessarily attendant upon it: a few hours work at any time would bring on an attack of this malady. Finding himself unable to support his family in this way, he became the teacher of a country school and continued in this employment nearly two years, when his mind became suddenly much affected; indeed for several days he had been quite insane. In this

wretched situation I was called to see him, when I immediately prescribed pills of calomel, aloes and rhubarb, as many to be taken every night as would operate two or three times the next day, and this course to be continued at least three months, which has resulted in a most happy manner. He discharged an incredible quantity of dark bilious matter, and expressed himself much mended after the free operation of the first pills. One year has now elapsed without any return of this disease, and nine months since the discontinuance of all medical treatment. He commenced work in his shop in six weeks or two months after he began to take the pills, and has continued in this employment ever since without any inconvenience.

The rationale of the above mentioned treatment is so familiar to all who have heard your lectures on this subject, that it would be idle in me to attempt to say much in relation to it. That venous congestion had much to do in the production of this disease, is quite evident from a circumstance noticed by the patient himself. In one of my interviews with him, he observed that a short time before an attack of the convulsion, he felt something crawling up on each side of his neck like a large worm, and that the veins became much enlarged about the neck. It is evident that this was nothing more than an enlarged and congested condition of the jugulars, in consequence of an obstruction to the free return of blood.

June 20th, 1830.

ART. VIII.—*Case of Paralysis of the Inferior extremities succeeding to Fever.* By CHARLES B. RAINES M. D. of Jasper, Tennessee.

ON the 30th of April 1830, I was called to visit Miss T— B— aged 18 years; rather of a bilious temperament. About two days previously she had been attacked by a very malignant bilious fever, for which, she was vomited once, and bled copi-

ously. Under this treatment, as I learned, the fever readily subsided. On the morning in which I first saw her, in an attempt to raise from bed she found herself entirely unable to use her inferior extremities; and the paralysis extended upwards rapidly, as high as her hips; all power of locomotion was entirely lost. The usual sensibility of both extremities remained, except occasionally a pinching sensation over the surface. She rested perfectly well at night; had no pain in the extremities affected; her skin was dry, but about the natural temperature; tongue coated with a dark brown fur; bowels obstinately constipated; and pulse full and strong. Her appetite was as good as in health, and she complained of no pain; and indeed of nothing but the inability to move her lower extremities.

I gave her eight pills formed of calomel, aloes, and rhubarb, and directed them to be repeated every six hours, until bilious evacuations were excited. After this had been effected, three or four of the pills were given every night, which kept up a fine bilious purging. She was ordered to be placed on a table, and the muscles of the inferior extremities were exercised by an assistant for two hours every day, by flexion and extension. On the 2d of May I drew ℥xij of blood, which was accompanied with a most salutary effect. Frictions with volatile liniment were freely used over the superficies.

May 7th, bled again to ℥xij; and the use of the pills, exercise, frictions, &c. were kept up for 24 days, with abstinence in diet, when the paralysis gradually yielded, passing off from the hips towards the toes; and on the first of June she was entirely well.

During the treatment of the case I perceived that as soon as the pills operated freely, the powers of locomotion seemed to be returning, and the paralysis subsiding; and as soon as that was neglected, or the pills failed to purge her during time, the paralysis evidently increased, and extended rapidly upwards to the body.

This case evinces the strong relationship existing between the diseases of the sanguiferous and nervous systems. There can exist no doubt but that in this case, the paralysis depended solely on the proximate cause of the fever, viz. congestion; and

serous effusion may have taken place, in a slight degree, in or about the lumbar or sacral nerves; and probably in the inferior part of the spinal marrow. But may not the paralysis have arisen from the pressure of the congested blood on those nerves? At least we observe, that as soon as the bloodvessels were partially emptied, and secretion was established, the nerves ceased to complain.

MISCELLANEOUS INTELLIGENCE.

Pathological Researches on Inflammation of the veins of the Uterus, with additional observations on Phlegmasia Dolens; by Robert Lee, M. D. Physician-accoucheur to the British lying-in Hospital. Read 13th and 27th October, 1829.—In a former communication, which I had the honour of presenting to this Society, on the pathology of Phlegmasia Dolens*, I was led, from a series of facts, to infer, that inflammation of the iliac and femoral veins gives rise to all the phenomena of that disease in puerperal women. Subsequent dissections have enabled me, not only to confirm the accuracy of my former observations, but have led me to discover the important pathological fact, that in Phlegmasia Dolens, the inflammation commences in the uterine branches of the hypogastric veins, and subsequently extends from them into the iliac and femoral trunks of the affected side.

The object of the present communication is, to submit to the consideration of the Society the various facts which appear to establish the truth of these general views of the nature and origin of Phlegmasia Dolens; and to detail the observations I have made on inflammation of the uterine veins.

[Here follows a number of Cases illustrating the views of the Author.]

All the different authors who have treated of Phlegmasia Dolens describe it as commencing, in the great majority of cases, subsequent to the 10th day after parturition, with symptoms of uterine irritation, and constitutional disturbance of a low nervous character, and with pain and swelling in one extremity only. They have assigned various reasons for these remarkable peculiarities in the period and mode of developement of the disease; as pressure of the gravid uterus on the iliac veins during gestation, the change in the distribution of the blood from the sudden removal of this pressure, exposure of the extremity to cold, suppression of the lochial discharge, &c., all of which however, taken singly, or combined, are insufficient to account for the phenomena: and the occurrence of the disease after menstruation and abortion proves that these causes are not necessary for its production. The facts

* See page 132.

which have now been stated afford a more satisfactory explanation of these phenomena, and shew, that if inflammation be excited in the orifices of the uterine veins, it may spread along these to the iliac and femoral veins, and by the morbid changes induced in them, give rise to all the subsequent symptoms.

The mode of developement and extension of the inflammation from the uterine to the iliac and femoral veins of the affected extremity, will be best understood by a concise statement of the principal facts relating to uterine phlebitis, of which Phlegmasia Dolens must now be considered as merely one of the remote consequences.

When the contents of the gravid uterus have been expelled in parturition, the orifices of the uterine veins, where the placenta had been attached, are left open, and a communication is indirectly established between the venous system, and the atmospheric air, in a manner somewhat analogous to what takes place in amputation, and other extensive wounds. Such a condition of the uterine veins, in consequence of the separation of the placenta, must be favourable to the production of inflammation; and inflammation once excited is seldom limited to their orifices, but extends with greater or less rapidity along the continuous membrane of the uterine veins, to the spermatic or hypogastric veins, and from thence to the vena cava, and its principal branches returning the blood from the lower extremities.

The effects of inflammation in the uterine veins are the formation of adventitious membranes on their inner surface, and the deposition of coagula of lymph or of purulent matter within their cavities.

Coagula of the fibrine of the blood, which often extend a considerable distance into the uterine veins, are formed in their orifices after every labour, and are the principal means employed by nature for the permanent suppression of uterine hemorrhage. These may be distinctly perceived for a long period after delivery, and they have a form and colour different from the coagula produced by inflammation. In opening the body of a patient four weeks after confinement, I observed distinct traces of these partially absorbed coagula in the muscular substance of the uterus, at the part where the placenta had adhered.

The inflammation may be limited to the veins, but not unfrequently the muscular tissue of the uterus contiguous to them participates in the inflammation, and becomes of a dark red or blackish brown colour, and so soft in its consistence as to be readily torn with the fingers. The

peritoneal covering may be also affected, and the usual consequences of puerperal peritonitis will then ensue.

The veins which return the blood from the uterus, and its appendages may be either wholly or in part inflamed; generally, however, and this is a circumstance in the history of uterine phlebitis deserving particular attention, the inflammation attacks the spermatic veins alone, and for the most part the one only on that side of the uterus to which the placenta has been attached; and it may either confine itself to a small portion of the vessel, or extend throughout its whole course, from the uterus to the vena cava. The usual consequences of inflammation of veins are then apparent, viz. injection and condensation of the cellular membrane in which they are imbedded, thickening, induration, and contraction of their coats, and the deposition of lymph, mixed with pus and coagula of blood within their cavities.

The same is the case with regard to the hypogastric veins, one only being generally affected. These veins are however rarely inflamed in comparison with the spermatic, and this would seem to depend on the latter veins being invariably connected with the placenta, to whatever part of the uterus it may happen to be attached.

But inflammation having once begun, it is liable, as I have before stated, to spread continuously to the veins of the whole uterine system, to those of the ovaria, of the Fallopian tubes and broad ligaments. The vena cava itself does not always escape, the inflammation spreading to it from the iliac or from the spermatic veins. This occurrence seldom takes place to a great extent through the medium of the spermatic, the inflammation usually terminating abruptly at the opening of the spermatic into it on the right side, or of the renal on the left. If it pursue, as it sometimes does, the direction of the kidneys, the substance of these organs, as well as their veins, may be involved in the disease.

Uterine phlebitis appears to result from the mechanical injury inflicted by protracted labour, from the force required for the extraction of the placenta in uterine hemorrhage, from retained portions of placenta undergoing decomposition in the uterus, the application of cold and probably of contagion, and from various unknown causes operating on the uterine system after delivery.

It is perhaps impossible to determine for the most part, the precise period of its invasion, from the total absence of local pain, and of other symptoms; but it is probable that it most frequently begins soon after

delivery, and remains stationary for a time around the orifices of the uterine veins, as phlebitis has been observed to do, where it occurs after venesection. Of this, however, we can have no certain proof, nor can it be admitted to be a general occurrence, from the rapidity with which the inflammation has been found to attack the uterine, spermatic, and renal veins. In one case the disease proved fatal on the evening of the fifth day after labour, and on dissection, all these veins were found disorganized.

It may be stated, as the general result of all the observations hitherto made on uterine phlebitis, that it occurs most frequently from the 10th to the 20th day after parturition, though it has been observed to commence at an earlier, as well as at a much later period.

Where the veins alone are inflamed, the peritoneal and muscular tissues remaining unaffected, there is often either no pain or only a dull pain, with a sense of weight in the region of the uterus; and no other local symptom by which the disease can be recognized. The uterus too may return to its usual reduced volume or nearly so, and it is only on the accession of the constitutional symptoms, which have been already detailed, that the existence of this insidious and dangerous affection can be determined. If the substance of the uterus be affected, this organ remains above the brim of the pelvis, large, hard, and painful on pressure, as in puerperal peritonitis.

With regard to the lochial discharge, it has sometimes been observed to be fetid, and puriform, and at other times in a perfectly natural state.

The constitutional symptoms of uterine phlebitis, and the important alterations which take place in the structure of the brain, lungs, and other internal organs, and also in the synovial, serous, and cellular membranes, are often so peculiar and characteristic, that it is the more remarkable this disease should have been so long overlooked by pathological enquirers. Mr. Wilson and Dr. Clarke, it is true, many years ago, observed pus in the veins of the uterus, and the same fact was repeatedly stated by others; but they pointed out merely the lesion of the uterine veins, without having observed the important constitutional effects to which the disease gives rise. In the cases which came under my observation in 1827 and 1828, and the histories of some of which were read by Mr. Arnott, before this Society, last season, some of the more remarkable of these general effects were distinctly stated, and many

others have, since that time, been pointed out by Messrs. Dance* and Cruveilhier†.

Recent experience has induced me to believe that uterine phlebitis is of far more frequent occurrence than has yet been suspected, and that to it must be referred many of the fatal disorders of puerperal women which have usually been comprehended under the vague designation of puerperal fever or peritonitis.

Inflammation of the uterus and its appendages may be considered as essentially the cause of all the destructive febrile affections which follow parturition; and the various forms they assume, inflammatory, congestive, or typhoid, will probably, in a great measure, be found to depend on the serous, muscular, or venous tissue of the organ having become affected.

In order still further to illustrate the observations which have now been made on uterine phlebitis, I subjoin an account of several examples of the disease which have come under my own observation, and in which some striking peculiarities occurred.—*Medico Chirurgical Transactions, Vol. XV. part II.*

On the effects of Tobacco, externally applied in cases of Strangulated Hernia. By William Maclay Awl, M. D. Somerset, Ohio.—I spent an evening in 1827 in company with several medical friends, and among other subjects of professional conversation, that of hernia was one: while considering the use and abuse of the tobacco enema; and particularly its occasional dangerous consequences; it was suggested by doctor Luckey, that its external application had some power; and the hint proving acceptable; it was agreed upon further examination of the known therapeutic effects of the article, that we would severally give it a trial, in the very first case of strangulation that should occur in our practice. Doctors William N. Luckey, Thomas H. Gibson, Eli Drasbaugh, and myself, have since that occasion, each met cases; and communicated one to another the fortunate, prompt, and decided success of the experiments without a single failure.

My first case took place on the 25th of March, immediately then ensuing, and is succinctly as follows:—T. C Esq. aged forty-five, a strong farmer; for some time subject to reducible hernia, sent for me

* Archiv. General. de Medicine. Tom. XV.

† Anatomie Pathologique, 4tieme Livraison, par M. Cruveilhier, 4to. Paris, 1829.

in the greatest possible haste this morning—and having but three miles to ride, the next half hour brought me to his chamber. He was supported on the edge of the bed, doubled up in agony; his countenance pale, cadaverous, and covered with heavy drops of cold sweat. The hands were occupied in supporting the painful scrotum, and the cold trembling frame told how near was the impending dissolution. "You cannot save me doctor," were his first hurried remarks; "it has been down six hours, further than ever; every thing has been tried, and it only grows more swelled and more painful every moment. You cannot save me, but you will exceedingly oblige me by writing my will."

Upon an examination I found the scrotum distended apparently to its utmost; tense as a drum head; highly inflamed, and so painful from ineffectual application of the taxis, that, the slightest movement was insufferable. The stricture could be plainly discerned through the integuments, and felt like a strong fibrous cord or band drawn across as with a machine; the circumambient parts, all swelled and painful. He had been vomiting, and hiccough began to show itself; pulse was missing.

I procured a large quantity of old strong tobacco (Kentucky plug) poured boiling water upon it, unfolded the leaves, and commenced lapping them round the tumor as warm, or rather as hot, as the patient could bear—having encompassed the entire bag with the tobacco three or four times double, I next spread of the leaves upon the surrounding parts and lower part of the abdomen in particular. In ten minutes the pain began to slacken; the usual sickening effects ensued very rapidly, and in a short time the soreness disappeared entirely. Uncovering the tumour it was found more pale and relaxed, but the stricture firm as ever. I again wet the leaves with warm water and added more of them; and covered the whole more closely, to prevent rapid evaporation. After a time the general languor of the nauseous weed wore away, and did not return, nor did the pain. Upon a second inspection there appeared to be no stricture remaining, and the hernial tumour was easy, soft, and loose. I gently compressed the scrotal pouch within my hand, and the bowel retired into the abdomen.

Remarks. I have but few remarks to make upon this case—it will speak for itself, and requires no explanation, other than to observe that tobacco has a peculiar topical relaxing effect *independant of its constitutional and usual prostrating effects*, as it was attentively remarked, that these wore away before the local action of the article, constituting

its direct importance, ensued. The joy which I experienced on the success of the experiment, was much alloyed by the reflection, that for want of this simple information, I had previously used the knife in cases, that would, under the tobacco management, have yielded as easily as did this one.

This information is perhaps given before its time, as I had intended at one period, to have waited for more cases, and have extended the use of tobacco externally, to some other affections, particularly tetanus, which, I feel confident it will cure, as likewise certain spasmodic conditions of the glottis—perhaps useful under certain circumstances of inverted uterus. However, be all this as it may hereafter, I at this time can vouch for the efficacy of tobacco in cases of hernia.—*Maryland Medical Recorder, No. 2, vol. 1. Jan* 1830.

Poisonous properties of the Cerbera Tanghin.—"The kernel of the fruit," Mr. TELFAIR writes in his letter from the Mauritius, dated March 8th, 1829, "must be a very powerful poison. It is not much larger than an almond, and yet is sufficient to destroy above twenty persons. RADAMA, the late king of Madagascar, abolished the use of it as an ordeal; whether the custom has been revived by the new government I know not. It was with great difficulty that the chieftains could be persuaded to admit of the abolition of an usage, which had existed from time immemorial, and whose unerring efficacy in the detection and punishment of crime, had never been questioned, until Mr. HASTY, our Government agent, had acquired such an influence with RADAMA and his court, as to admit of the exposure of its fallacy. But this was the work of years; and although Radama was at length himself convinced that nothing could be more unjust, than the continuance of the practice, he dared not so far shock the prejudices of his people, as to order that it should cease. Even the chief performers in the ceremony, the "Skids," as they are called at Tanararissoo, who unite in their own persons the offices of priests and physicians, and who administer the poisonous kernel to the victims, never doubt its power of revealing guilt or clearing innocence. The last occasion on which it was practised in Radama's reign, and of which he availed himself to effect its discontinuance, personally regarded his court and attendants. The king was affected with a complaint of the liver, for which the "skid" prescribed some inefficacious remedies, and as the disease became worse, Mr Hasty gave him some calomel in doses, which he had

found by experience to relieve himself under similar symptoms. The disease disappeared, but ptyalism was produced, and alarmed the king's family, who believed that he was poisoned, and insisted that all his immediate attendants should be put to the ordeal of the *Tanghin;* and the royal Skid was most earnest in pressing to have it performed, although he himself from his rank and place was among the first to whom it would be administered. In vain the king protested that he felt himself cured, and that the indisposition and soreness of the mouth was caused by the medicine that had relieved him, and which would pass off in a few days. The Skid insisted, the ministers and principal chieftains joined with the family in requiring the ordeal; to which the king, in spite of his convictions, was compelled to consent; but at the same time, he made it a condition that this should be the last exibition of the kind, and he bewailed the necessity which deprived him of so many attached dependants, whose fate he had predicted, while he protested his conviction of their innocence.

The king's servants, including the Skid, were more than twenty in number; they were shut up at night separately, and not allowed to taste food; the next morning they were brought out in procession, and paraded before the assembled people; the presiding Skid had the *Tunghin fruit* in readiness; after some prayers and superstitious evolutions, he took out the kernel, which he placed on a smooth stone, and with another stone broke down a part of it into a soft white mass like pounded almonds. The victims were then brought separately forward, each was questioned as to his guilt, and if he denied, his arms were tied behind, and he was placed on his knees before the Skid, who put a portion of the pounded kernel on his tongue and compelled him to swallow it. Thus the kernel was shared among all the king's personal servants. On some of the individuals the action began to operate in half an hour or less. The Skid takes particular notice how they fall, whether on the face, to the right or left hand, or on the back, each position indicating a different shade of guilt. Convulsions generally come on, accompanied with efforts to vomit. Those whose stomach rejects the dose at an early period, usually recover; on this occasion there were only two individuals with whom this was the case; the others were thrown, in a state of insensibility, into a hole, and every person present at the ceremony was obliged to throw a stone over them, so that their burial was quickly completed. The king's Skid was one of the first that fell. Those that recover are supposed to bear a charmed life ever

after, and are respected as the peculiar favorites of the gods."—*Curtis's Botanical Magazine, February*, 1830.

Some Account of a Case of Paruria Inops, (Good,) or Paralysis of the Kidneys. By George Hayward, M. D. of Boston.—This disease, in which, according to Dr. Good, the "urine is unsecreted by the kidneys," and there is "no desire to make water, nor sense of fullness in any part of the urinary track," is of very rare occurrence. No writer but Sir Henry Halford, that I am aware of, has published any account of it; this circumstance, together with the fact that its termination is usually, if not always in death, induces me to submit the following details of a case that recently occurred in my own practice.

On Thursday, July 16th, 1829, at 1 o'clock P. M. I visited a lady in the fiftieth year of her age, the mother of several children, who complained of considerable nausea, with diarrhœa and slight pain in the stomach and bowels. She had been as well as usual till Tuesday evening, but since that time had been so much indisposed as to abstain from all food. Her indisposition she attributed to taking cold, from exposure on Monday night. She had formerly been a good deal of an invalid, having suffered severely from repeated miscarriages, but had for the last eight or ten years enjoyed a very tolerable share of health.

I found her tongue covered with an unusual thick coat, her pulse between seventy and seventy-five in a minute, moderately strong, and her skin cooler than in health. I directed a gentle emetic of the wine and powder of ipecacuanha, to be followed by castor oil, and the dejections to be restrained by opium, if they were excessive.

On Friday morning I learnt that the emetic had operated thoroughly but mildly, and that she brought from her stomach food in an undigested state, that was taken on Tuesday. Her bowels had been so frequently moved as to render it necessary to give her three grains of opium at intervals. She was somewhat stupid, which at the time was attributed to the opium; the coat on the tongue remained about the same. She still complained of nausea; though she was free from pain; the pulse was slower than on the preceding day, and the temperature of the skin was diminished. At this visit, she told me that she had passed no water since early on Wednesday morning, but that she had no desire to do so, and no pain or inconvenience from it. On passing my hand over the bladder, I satisfied myself that it was distend-

ed; I directed her to take one drachm of a mixture of three parts of the liquid acetate of ammonia, and one part of the spirit of nitrous æther, every two hours, and to let me know in the afternoon if she had not evacuated the bladder in the interval. I was sent for in the afternoon, as no water had been passed; there was still no suffering, and the bladder was not distended. I then introduced the catheter, and drew off about half an ounce of urine of a very healthy character. The patient was more drowsy at this visit than I had seen her at any previous one, and being now convinced that the whole trouble arose from a want of secretion of urine, I stated to her family that I considered her situation an alarming one, and that the disease would probably have a fatal termination. This surprised them, as her strength was good, she was without pain, and conversed freely, when roused from the stupor to which she was inclined.

I now directed a large blister to be applied over the kidneys, fomentations of hot herbs in spirit above the pubis, sinapisms to the feet, and stimulating frictions to the whole surface of the body, with a continuance of the diuretic mixture.

On Saturday morning all her symptoms were aggravated; the pulse slower, the skin colder, and the coma increased. The tongue remained coated, there was no appetite for food, and no water had been passed. A powder, composed of one grain of the sub-muriate of mercury, five grains of the nitrate of potash, and a scruple of cream of tartar, was ordered to be given every two hours, and the medicine that had been before directed was to be taken in the intermediate hours, and the other remedies were continued. No improvement took place during the day; on the contrary, the coma increased, the pulse became slower and more feeble, and the temperature of the skin was diminished.

Finding all her symptoms worse on Sunday morning, I directed ten drops of the tincture of cantharides and capsicum to be given every two hours, instead of the mixture of the spirits of nitre and Minderus, and the other remedies to be continued. At this visit I passed the catheter, and drew of about an ounce of healthy urine. At three o'clock P. M. Dr. Warren saw her with me; she was now so comatose that it was impossible to rouse her, and her pulse had sunk very considerably since morning.

Dr. W. advised to give one drachm of the tincture of cantharides and capsicum every two hours, to rub along the whole course of the spine with the same, and to continue the use of the other means. The

medicine was given and the other directions followed till eight o'clock in the evening, when she became unable to swallow, her pulse ceased at the wrist, the surface of the body became cold, and the breathing stertorous, and at long intervals; and in this state she continued till Monday evening, at seven o'clock, when she died.

Sectio cadaveris, twenty-three hours after death.—The examination was made in presence of my friend, Dr. Homens, of this city.

The general appearance of the body was natural. On dividing the scalp from ear to ear, and dissecting it from the cranium, no fullness was discovered in the vessels of the integuments, and scarcely any blood was effused. The brain and its membranes were found to be in a perfectly healthy state, there was neither effusion nor congestion, but all the appearances warranted the conclusion that the morbid symptoms were owing to the quality of the blood, rather than to its quantity.

There was no mark of disease in the stomach, intestines, liver, spleen, or uterus. The kidney of the right side was about half the usual size, and a third part of it at least was of a deep purple colour, exhibiting traces of considerable inflammation, apparently recent. When cut into, it emitted a strong urinous odour.

The left kidney was not larger than a small English walnut, but of a healthy appearance, and free from any urinous odour. Both the ureters were somewhat inflamed The bladder did not contain a drop of urine: the mucous coat was nearly black, appearing to have been the seat of violent inflammation. Whether this was the case, or whether the inflammatory appearance about the ureters and bladder was to be attributed to the absence of urine, the usual stimulus of the parts, is a point which I feel unable to decide.—*Amer. Jour. Med. Sciences, No. IX.*

To the above interesting case of a rare and generally fatal disease, we are enabled to add another which came under the observation of the Junior editor of this Journal a few years since.

A gentleman aged about seventy years, of a large frame, corpulent and gouty, had been for many years troubled with a calculous affection and occasionally voided *per urethram* small quantities of gravel. He became slightly indisposed with the ordinary symptoms attending this state of things, a dull pain in the region of the kidneys, gastric uneasiness &c.; and an obstruction of urine having taken place, which did not yield to the remedies which he was in the habit of resorting to, such as bleeding, purging, saline diuretics &c. he sent for a physician,

who seeing no urgent symptoms present, did nothing more than order some more efficient diuretic medicines, which he did not doubt would carry off the obstruction. These medicines were used diligently for twenty-four hours, with no manifest effect, when I was desired to visit him and draw off his water—He had then passed none for three days and as many nights; and yet he complained of no uneasiness in the pubic region, nor was any unusual distention perceivable. A moderate sized silver catheter was introduced into the bladder with perfect facility, and to my utter astonishment not one drop of urine passed through it.—The instrument was withdrawn and a large flexible catheter of gum elastic, was substituted, having much larger perforations at the extemity. The result, however, was the same, no urine whatever was evacuated. This patient died comatose at the end of the fourth day, without fever, without pain and indeed without complaint of any thing, and the only striking feature of his case appeared towards the close of it to be that heavy, lethargic somnolency, so particularly alluded to in the cases of Sir Henry Halford and Dr. Hayward. No post mortem examination of the body was made. S.

Remarks on the use of Spirit of Turpentine in Incarcerated Hernia. By C. B. Hamilton, late Surgeon of the Marine Hospital at Washington City.—Being called to a case some years ago, of strangulated scrotal hernia of but a few hours standing, which, from the great distention of the strangulated bowel by flatus and excrement, resisted all my efforts at reduction by taxis, I was naturally led to speculate upon the cause of so great and sudden an accumulation in the gut. It struck me that if the occluding stricture existed in the abdominal ring, it must necessarily act alike upon the descending and ascending portions of the intestine, and that of course nothing could be derived to the incarcerated portion from that within the abdomen, to give it the volume it possessed. It therefore occurred to me that the descending portion of the tube was free, and that the distention was caused by a stricture taking place in the muscular fibres of the ascending portion, and arresting the passage of the contents of the bowels brought down by the peristalic motion. Considering this state of things to differ in no particular from that which takes place in spasmodic colic, I at once resolved to make trial of the turpentine, the good effects of which I had so often witnessed in the latter disease, and it succeeded beyond my most sanguine anticipations. In a few moments the con-

tents of the strangulated bowel were spontaneously removed, and the intestine restored to the abdominal cavity by taxis, with perfect ease.

About twelve months since, I was called to a coloured man, the property of John Addison, Esq. of this district. On my arrival, I was informed by his master that he had been for many years afflicted with scrotal hernia, that he had been in the habit of reducing it himself, that a few hours before he had been seized with severe pain in the part, and that the rupture now resisted his usual efforts to reduce it. On examining the patient, I found the scrotum so enormously enlarged that no trace of a penis could be seen; the integuments were cold to the touch, and the swelling elastic. The patient informed me that a short time before the attack of pain, he had eaten a quantity of unripe fruit, and ascribed his situation to that cause. Without making any attempt at reduction, I enquired if there was any spirit of turpentine in the house, fortunately about the half of a common-sized wine-glassful was produced, which I immediately administered. The relief was instantaneous: the spasm was removed; the air and fæces, by the elastic pressure of the intestine, was carried upwards with a gurgling sound into its continuous portion within the abdomen, and in five minutes after, the patient with his own hand, reduced the rupture.

I have made these remarks for the purpose of directing the attention of practitioners to what I consider to be the only condition of the parts, (which, by the way, might, I conceive, be properly termed a scrotal colic,) in which the turpentine proves an invaluable remedy, and to express my opinion of the impropriety of administering it in those cases where the obstruction arises from a stricture of the tendon forming the abdominal ring, or from chronic enlargement of the incarcerated viscera.—*Amer. Jour. Med. Sciences. No. IX.*

On the Mode of determining by the decay of the Body how long it has been in Water. [*Annanels d'Hygiene Publique et de Medecine Legale, Octobre* 1829.] *M. Alphonse Devergie*, who has already distinguished himself by several interesting essays on various subjects connected with medical jurisprudence, has recently turned his attention to the appearances in the bodies of the drowned which indicate the interval during which they have remained in water. This is often an extremely important point to determine on coroner's inquests and precognitions, and no precise information has hitherto been possessed regarding it; while at the same time it appears from the researches of

M. Devergie to be always susceptible of very exact determination from characters almost invariable in the order and rapidity of their developement. His researches were conducted by permission of the Prefecture of the police at Paris, at the Morgue of that city, the nature of which establishment we need scarcely stop to describe. His attention has been hitherto confined to the appearances developed at low temperatures, namely in the months of January, February, March, and April, when the mean temperature was 28½, 35½.41, and 48 F. But he promises to give an early account of the modifications induced by increased temperature. His observations were made on 62 bodies, of which 45 were recognized by friends, so that the duration of their immersion in the water could be distinctly ascertained.

For the most part no change is observed before the 4th day. The rigidity which supervenes soon after death often continues till the second or third, sometimes till the fourth. On the *fourth day* the outside of the palms of the hands and the points and sides of the fingers begin to be—blanched, the backs of the hands and the rest of the body continuing unaltered.—About the *sixth or eighth day* the cuticle on the backs of the hands begins to whiten, as also that on the soles of the feet. At the same time the skin of the face is softened, and its whiteness is more dull and opaline than the rest of the skin.—*About the fifth day* the face is slightly puffed up and reddish here and there; a greenish tint appears over the middle of the sternum; the cuticle of the hands and feet is entirely white, with the exception of the upper surface of the feet; the cuticle of the palms has begun to shrivel; the subcutaneous cellular tissue of the chest is red; and the cortical substance of the upper part of the brain is greenish.—*In one month* the face is red and brownish, the eye lids and lips green and swelled, the cheeks greenish and also swelled, the parts around the eyes and nose brownish, the nose reddish-brown, swelled in women, but commonly flattened and depressed in men. The last point of difference depends on the circumstance that the bodies of females lie in water with their faces uppermost, and those of males with the face undermost. A reddish brown patch, six or eight inches broad, and surrounded by a green border occupies the middle and upper part of the sternum. The scrotum and penis are enormously distended with gases. The cuticle of the hands and feet is extremely white and wrinkled, exactly like the effects of a poultice when long applied to a whitlow. The hair and nails still adhere firmly. The cellular tissue is very red. The lungs

are very emphysematous, and extend over a considerable part of the pericardium. The windpipe no longer contains froth, which has been all expelled by the disengagement of gasses in the lungs.—*In a month and a half*, besides the changes already mentioned, the neck and sides of the cheeks have acquired an intense green colour; the subcutaneous cellular tissue is still redder than before; and the cuticle begins to separate from the base of the hands; but the nails still adhere firmly.—*In two months* the whole body is covered with a slimy mud composed of extremely fine particles, and which oozes through the clothes. The period at which this appearance takes place is earlier in muddy water, and later in water which is much agitated. The face is much swelled, universally brownish, the lips very large and retracted from the teeth, the cuticle raised here and there in vesicles on the face, and every where easily detached, the course of the veins on the forehead strongly marked by blue lines. The greenness on the sides of the chest has extended upwards to the shoulders and downwards along the flanks to meet similar green marks proceeding from the groins; but the middle of the belly, as well as the arms, fore-arms, thighs, and legs retain their natural colour. The cuticle of the hands and feet is raised up and detached; and the nails are sometimes partially raised from the fingers, but adhere to the cuticle. The hair begins to drop away, and is easily plucked out. The cellular tissue is reddish brown. The veins are distended with air and almost free of blood, but the arteries contain much bloody fluid. The windpipe is white over the cartilages, reddish-brown in the interstices between them. The heart is soft, flaccid, and empty; and if the right cavities contained blood at the time of death, the lining membrane of these cavities is jet-black, while that of the left cavities is reddish. The stomach and intestines are intensely red, like the effects of inflammation. A considerable quantity of bloody fluid is effused into the pleural and peritonæal sacs.—*In two months and a half* the green tint of the skin is extended over the arms, fore-arms, and legs, and also in patches over the back. The nails of the fingers are commonly detached, but very rarely those of the toes. In females the state of the body is somewhat different at this period, in consequence of the greater abundance of fat under the skin. The skin of the face is white and opaline, and here and there eroded in the small points. The breasts are much swelled, and of an opaline whiteness, while the nipples are brown, small, and flaccid. The belly is distended, also of opaline whiteness, together with the thighs, arms, and legs. The sub-

cutaneous cellular tissue is converted into adipocire in the middle of the cheeks, under the eyebrows, on the chin, in the upper part of the neck, and very superficially in the breasts, fore-part of the thighs, and arm-pits. The muscles retain their natural colour and texture.—*In three months and a half* the face is so altered, that the age of the person is not ascertainable, the scalp is destitute of hair and cuticle, is soft, and at the forehead thin or partially destroyed. The eyelids are in a great measure destroyed, the eyes sometimes protruded, sometimes shrivelled up, the skin over the cheeks and chin opaline, and covering a layer of adipocire, the lips depressed and saponified, the middle of the belly opaline, and interspersed with eroded spots. Erosions varying in size from that of a half-penny to that of a penny are dispersed over the skin of the arms and fore-arms, of a roundish form, and thin on their edge; and larger erosions of the same nature appear on the front of the thighs and legs. Those over the tibia lay bare the bone, which is red; and there the corrosions have a close resemblance to denuding ulcers formed during life. The cellular tissue at the bottom of these spots is saponified or converted into adipocire, which change, indeed, has also by this time occurred in the whole subcutaneous cellular tissue of the neck, groins, and fore part of the thighs. All the superficial muscles have acquired a rosy tint, and soft, flaccid consistence. The lungs no longer fill the chest, a serous fluid being effused into the pleural sac. The liver is softened, and of a greenish-brown tint, the gall-bladder empty, the intestines deep reddish-brown. *In four months and a half* the scalp is detached from the bones, and almost entirely destroyed over the forehead, the eyebrows are also destroyed, and likewise the soft parts of the nose, and the lips are completely disorganized, so as to expose the whole alveolar part of the jaws. The skin of the neck and trunk is greyish-green, with black spots interspersed; that of the thighs is yellowish and firmer, and the skin in various parts presents a series of little, indurated, mammilliform elevations. The cellular tissue of the face, neck and fore part of the thighs is completely saponified; elsewhere, it is distended by gases The eroded spots of the skin are more extensive, and usually lay bare the whole tibia. The brain is partly putrified, partly converted into a matter resembling the adipocire of dead bodies.

M. Devergie has not yet had sufficient opportunities of ascertaining the changes occurring at precise periods later than four months and a half. But the following general view of the ulterior transformations

which the body undergoes are worthy of notice. In the body of a woman who appeared to have been from five months to five and a half in the water, he found the whole head completely saponified, the cheeks so hard that they yielded with difficulty to the finger, and covered with a calcareous-looking deposit. The skin and muscles at the union of the neck with the trunk were destroyed; but a great part of the skin over the middle of the trunk was entire, having been protected by stays. This he has often had occasion to observe. The skin in many places was covered with little tubercles encrusted with calcareous salts, and in some parts hardened into a substance like parchment. In the body of another woman which had been apparently from ten to twelve months in water, the whole bones of the skull were bare and very brittle, the orbits filled with adipocire, the teeth gone, the fore-arms and hands gone, and the legs and feet reduced to the bones only. The skin on the fore-part of the body was so hard as to ring when struck with a key or scalpel, and covered with little hard tubercles, consisting of calcareous matter. The skin on the back was still soft, smooth, and free from tubercles. The whole cellular tissue was saponified, and was hard anteriorly, softer posteriorly. The muscles, which are contained in an aponeurotic expansion, retained their fibrous structure; the rest were saponified. The whole serous membranes had resisted decay, and had even become more solid than natural. The lungs were reduced to a tenth part of their volume, but were in a state of complete preservation, so that they admitted of being blown up to seven times the size of the space they occupied in the chest.

Among the appearances noticed in the preceding summary, two are especially worthy of notice,—the peculiar whiteness and wrinkling of the cuticle of the hands and feet,—and the slowness with which the green colour invades the rest of the body before it reaches the middle of the belly. These two characters will serve to distinguish a body which has putrified in the air from one which has decayed in water. In the air, the hands and feet never become wrinkled and white; but in water this change is brought on to such an extent, that if the body afterwards is allowed to putrify in the air, the earlier history of its decay may be long recognized by the state of the cuticle on those parts. As to the colour of the abdomen, this is the first part of the body which becomes green, when putrefaction takes place in the air; and the last where the greenness appears, if the body has remained in water.—*Edinburgh Med. and Surg. Jour. April* 1830.

Discovery of Arsenic in a body seven years after interment. (*Archives Gen. de Medecine Decembre* 1829.)—At the meeting of the Parisian Academy of Medicine on the 1st December last, *Professor Orfila* read an account of an important case in medical jurisprudence, which had been recently before communicated to him by *M. Ozanam*, chief physician of one of the hospitals at Lyons. The body had been buried in an elevated spot, and in a soil which absorbed moisture with great rapidity. Consequently it was found on disinterring it, that the coffin was entire, though dry and brittle, and that its sides were not even stained within, and the bottom only slightly stained with a brownish unctuous matter. The body was also entire; the head, trunk, and shoulders had preserved their form and position; but the internal organs of the chest and abdomen were destroyed, and there only remained a mass of soft brownish matter, which was deposited along the sides of the spine In this matter *MM. Ozanam* and *Idt*, who were appointed judicially to investigate the case, discovered arsenic.

A part of this matter was boiled in repeated portions of distilled water till the water ceased to carry away with it any impregnation. The solutions thus obtained were subjected to two series of tests. 1. One portion was evaporated to dryness and the extract re-dissolved. The new solution on being reduced by evaporation to the volume of two pounds still retained a deep-red colour; on which account it was again evaporated to dryness, and deflagrated with nitre according to process of *Rapp* (See this Journal, vol. xx.) The saline residue being dissolved, filtered, boiled, with nitric acid, and saturated with pure potass,—the tests for arsenic acted on it in the usual manner. In particular, the hydrosulphate of ammonia, with a drop of muriatic acid, caused a yellow precipitate soluble in ammonia; and another portion of the solution being precipitated with nitrate of silver, the precipitate was mixed with pure vegetable charcoal and a little potass, and heated in a tube, when the tube became covered as it were with a slight tinning, which, when examined with the microscope of Delabarre, appeared to consist of metallic tetrahedral crystals. When these crystals were placed in distilled water, and a stream of oxygen transmitted, they were dissolved, and the solution was acted on by the proper tests exactly as a solution of common white arsenic. 2. Another portion of the decoction of the original matter was treated with sulphuretted-hydrogen and a little muriatic acid; and the precipitate was reduced in a tube with charcoal and potass. A brilliant encrustation was procured, which

when treated as in the former instance, became a solution of protoxide of arsenic, as the proper tests indicated.

This is an incident of extreme importance. It shows that in some circumstances it is hardly possible to assign a limit to the period after death within which arsenic may be detected by chemical analysis. Instances have occurred in this country where arsenic was detected several weeks after interment. In Germany it has been discovered thirteen and fourteen months afterwards (See Dr. Christison's Treatise on Poisons, p. 261.) But no previous instance is on record of its detection after so lengthened an interval as in the case now described.

The method adopted by *MM. Ozanam* and *Idt* for determining the arsenical nature of the brilliant sublimates procured in their experiments is new. Doubts were expressed by several members of the academy whether oxygen transmitted through water, in which metallic arsenic was suspended, would have the effect of oxidating and dissolving it, as these two gentlemen alleged. But Professor Orfila, on repeating the experiment, found it perfectly correct. This might have been inferred from the fact that metallic arsenic is rapidly oxidated by contact with water in consequence of appropriating the oxygen held in solution by the water. The method here pointed out for converting the metal into the oxide is vastly inferior as a medico-legal test to the process proposed by *Dr Turner*, which consists in oxidating the crust and converting it into crystals of arsenious acid by the application of heat.—*Ibid.*

Importance of the Science of Pharmacy.—We extract the following from an article in the North American Medical and Surgical Journal, purporting to be a review of the first volume of the Journal of the Philadelphia College of Pharmacy.

To be convinced of our own inferiority, is the first step towards improvement: and the only way to arrive at this conviction, is to compare our attainments with those of others It will not, therefore, be unprofitable to devote a few pages to a very general sketch of the state of pharmacy as it exists in some of the countries of Europe. In the work before us we find all the materials for such a sketch, in an address delivered before the Philadelphia College of Pharmacy, by the President, Daniel B. Smith, and published in the fourth number of the journal.

In England the state of the pharmaceutic profession is peculiar. The apothecaries in that country have long been in the habit of practising as physicians, being called upon in trifling cases, or in the com-

mencement of diseases, or in circumstances where regular aid, either from poverty, distance, or other causes, is not attainable, and yielding to the regularly educated practitioner when the complaint assumes an alarming aspect. It is unnecessary to dilate on the evils which must arise to the patient from this anomalous mode of proceeding. One result, however, deserves particular notice. As the apothecary is allowed to charge only for the articles which he furnishes, and receives no compensation for his attendance, the temptation to prescribe an unnecessary and even injurious quantity of medicine is irresistible; and the consequence is an enormous consumption of drugs among the English. But in a country so far advanced in civilization, and where the extent of competition forces every art into the highest state of perfection, it would be very singular if a profession, demanding such high qualifications as that of pharmacy, should not attract the exclusive attention of a certain set of men. The fact is that, in all the cities and towns of Great Britain, apothecaries, in our own sense of the term, are to be found, who confine themselves to the preparation and sale of medicines. They are distinguished from the practising apothecaries by the title of chemists or druggists.

"Within the last forty years this class of tradesmen has greatly increased, and the principal shops of the kind in London are perhaps among the best regulated in the world. In one, especially, the most celebrated in that great emporium, the system of business is so perfect as to excite admiration. The shop is situated in a narrow court, the only access to which is through an arched alley. Before any article is allowed to be sold, it is examined and approved, and, if a chemical preparation, is tested by one of the partners, and no medicine is permitted to pass out of the shop but of the finest quality. Each clerk has his particular station at the counter, and the requisites of business about him, his own money drawer and medicine bottles, scales, measures, and papers, for the utmost cleanliness of all which he is accountable. He sets down all the money he receives as it is paid in, and keeps his own cash account. No conversation above a whisper is allowed except on business.

"A minute account is kept, and an investigation made at regular periods, by one of the proprietors, into all the incidents of the shop, the errors in putting up medicines, or in the cash accounts, and of the manner in which the rules of the shop, which embrace all the minutiæ of cleanliness and method, have been observed. Such strict

system and accuracy may be difficult to enforce, and may seem like an useless waste of time. But there is no such thing as too much system or accuracy in the business of the apothecary; and the great reputation of the shop to which I allude is a proof that it is profitable as well as creditable A thousand guineas, it is said, have been offered to the proprietors as a fee for an apprentice."

The following extracts will give an idea of the condition of pharmacy in Germany, and the neighbouring countries to the north and east.

"The apothecaries are limited in most of the towns to a certain number, so that the ownership of a licensed shop is of great value, and the strictest vigilance is used in the inspection and administration of medicines. The German apothecaries never contend with each other by underselling. They enjoy a consideration founded upon that which they pay to each other; their establishments are a fixed property, and there is neither rivalry nor collusion among them. Secure of realizing a fortune by their business, they introduce a rigorous method into all its details. Less splendid in the exterior of their shops than the French, they have more true solidity in the arrangements within, and their medicines are recommended by the excellence of their preparation. The German apothecaries have prevented their business from becoming purely mercantile. In the midst of wars and disorders they have profited by the discoveries and improvements of other nations, and have remodelled their dispensatories upon the most scientific principles. More copious than the English, and less prolix and complicated than the French, their pharmacy has adopted the best parts of the pharmacopœias of London and Paris, and is intermediate between these two systems. The Germans accordingly furnish apothecaries to many of the neighbouring countries. They enjoy the monopoly of the business in Prussia, and there are none but German apothecaries in Moscow and St. Petersburgh.

"In the latter city an apothecary dare not make up a prescription of any practitioner whose name is not in the printed list of physicians; nor can he venture to sell a drug, however small in quantity or insignificant in quality, without a prescription regularly signed; and every thing sent from his shop must be wrapped in a sealed packet."

"In Prussia the pharmaceutic code is exceedingly strict, and requires high qualifications in the apothecary. Its severity is particularly exem-

plified in the rigorous examination to which the candidates are subjected.

"The laws of that kingdom require that a candidate for examination must have served a regular apprenticeship of five years, or have been employed for three full years as an assistant in a licensed shop, and at the completion of either period, must have attended two full courses of lectures on botany, chemistry, natural history, pharmacy, and medical jurisprudence. The board of examiners is composed of two chemists and naturalists, and two scientific and practical apothecaries, who are paid by the government, and have had no part in the instruction of the pupil. The candidate is first obliged to translate passages taken at random from the Prussian Pharmacopœia, to satisfy the board of his skill in the Latin language. He must then write a Latin theme on two subjects of chemistry or medical jurisprudence, the titles of which are drawn by lot from an urn. This theme must be written in eight hours, in the presence of the examiners, without the aid of books, assistants or extracts. If he pass this ordeal, two difficult subjects of pharmaceutic or analytic chemistry are given to him, upon which he is obliged to write a theme at his own dwelling, with the aid of books, in order to prove that he has received the highest scientific chemical education. He then draws by lot two chemical or pharmaceutic substances, either natural or artificial, and is allowed eight days, at the end of which time he must have made a complete analysis of them, and written down the results of his experiments. He is also obliged to analyse the contents of the purposely poisoned stomach of an animal, and to write a juridico-chemical paper thereon. Not satisfied with so close a scrutiny, which would deter any apothecary in this country, from soliciting an examination, the candidate is then required to draw by lot the names of two pharmaceutic compounds of difficult preparation, which he is obliged to prepare in the presence of the committee extemporaneously. Specimens, fresh and dry, of officinal plants, ten samples of drugs, and several chemical preparations are then placed before him, which he must name at sight. He must then give [illegible] scientific descriptions of the plants and their uses; [illegible] origin, properties, and adulteration of the drugs, and the chemical [illegible] ments, mode of preparation, and [illegible] and the means of testing their purity.

"The examiners are [illegible] to be present throughout all these [illegible], and to keep accurate [illegible] of their proceedings, and of the [illegible]

or failure of each attempt. If they approve of the candidate by a majority of votes, he is admitted to the public examination, at which he must answer questions in chemistry, natural history, and medical jurisprudence; after which, if he is still futher approved of he is recommended to the minister of the interior, who gives him a licence to practice his art."

But it is in France that pharmacy has attained the highest eminence. We cannot offer a more compact view of the legal condition of the profession in that country, than is presented by the following extract from the address of Mr. Smith. The interest of the matter will be our best apology for its length.

"The French law regulating the sale and dispensation of medicines, establishes three schools of pharmacy—one at Paris, another at Strasburg, and a third at Montpelier. Each of these schools is obliged to open at least four courses of experimental lectures—one on botany, one on the natural history of medicines, and the other two on practical pharmacy and chemistry.

"The pharmaceutists of those cities are compelled to enter at the school the name, age, and other circumstances of their apprentices, who are all obliged to attend the lectures. An apprenticeship of eight years is required before a person is allowed to open a shop, except he has attended three courses of lectures in one of the schools, when only an apprenticeship of six years is exacted. The length of each course is five months, and a strict scrutiny is exercised over the attendance of the pupils. A prize is annually delivered at the expiration of the course for the best essay on any of the sciences taught in the schools.

"When a pupil wishes to become a licentiate, he is required to produce the certificate of the school where he has studied, and of the pharmaceutists with whom he has served his time, as well as an attestation of his moral conduct, signed by two resident citizens and two authorized pharmaceutists. He must also produce a certified copy of the register of his birth, to prove that he has completed his twenty-fifth year. If the director and professors of the school are satisfied with these documents, they appoint a day for the first examination. The student must undergo three public examinations, the intervals between which must be at least a month. One of these is on the principles of the art; another on botany, and the natural history of the materia medica; the third examination is on the practice of pharmacy, and continues for four days. It consists of at least nine chemical or pharmaceu-

tical processes, performed by the candidate in the presence of the examiners, to whom he must describe the materials, the operation and the results, and explain the rationale of the process. He must receive the votes of two-thirds of his examiners before obtaining his diploma.

"In those places where there is no school of pharmacy established, the examinations are conducted in the same manner by a jury composed of physicians and pharmaceutists, and no person is allowed to practice pharmacy unless licensed by a jury, or by one of the established schools. The licentiates of the schools can exercise their profession in all parts of the kingdom; those of the juries are restricted to the department in which they have been examined.

"No pharmaceutist is permitted to sell any secret medicine. At Paris, Strasburg, and Montpelier, an annual inspection of the shops and warehouses of the pharmaceutists and druggists is performed, by a board consisting of two professors in the medical school, the members of the school of pharmacy, and a commissary of the police. All deteriorated or badly prepared drugs are seized by the commissary, and the person in whose shop they are found is liable to a fine of one hundred livres, and an imprisonment not exceeding ten months. These annual visits of inspection are paid in other places by the juries who examine the candidates.

"Poisons are required to be kept in secure and separate apartments, of which the master of the shop alone keeps the key. They are to be sold to none but a known and resident person, under a penalty of three thousand francs; and all purchasers must write their names and residence, the quantity and nature of the poisonous drugs they have bought, and the purposes for which they are wanted, in a register open to the inspection of the police, under the same penalty."

Notwithstanding the excellence of most of these regulations numerous abuses have crept in, which have attracted the notice of the society of pharmacy at Paris, and called forth a memorial from this body to the chamber of deputies. We notice this circumstance in order to present to our readers an extract which exhibits the high, and at the same time just estimate formed by the memorialists of the general character of their profession, and of the attainments and standing of many of its members.

"The knowledge" say they "which pharmacy requires, without being as extensive, is in part the same as that which is necessary to the physician. It is as various, and is sufficiently useful to entitle him who pos-

sesses it to the particular attention of government, and to general respect. The pharmaceutists enrol in their number men of distinguished learning, who belong to the most celebrated academies; skilful professors, who fill the chairs of chemistry and natural history; writers, whose works are sought for in France, and abroad; respectable citizens, whose public services have been rewarded by honours, titles and decorations."

The state of the pharmaceutic art in Europe, as exhibited in the above extracts, compared with its present condition in the United States, presents a lamentable contrast to our disadvantage. The physician in most parts of the country, not only in villages or thinly settled places, where there might not be support for an apothecary, but in many of our large towns and cities, keeps, and to a certain extent prepares, the medicines he employs, the dispensing of which he is often compelled, in consequence of the burthen of his proper professional engagements, to leave to his students, whose only knowledge of the business has been acquired by the study of a few months in his office. Even where apothecaries have been established, they have frequently entered into the business with no other preparation than a short experience, obtained in the shops of others, sometimes without even this advantage. Provided with a dispensatory, and perhaps with a few other elementary works on pharmacy, without any knowledge, or at best with only a smattering of chemistry, they feel themselves competent to the performance of duties which in Europe are thought to require the previous devotion of many years to persevering study, and to the acquisition of practical skill.

This, however, is the dark side of the picture. It gives us much pleasure to be able to state that pharmacy has begun to partake of that spirit of improvement which pervades every art and profession that tends to administer to our physical comfort. In the city of Philadelphia, particularly, great changes for the better have taken place within a few years.—*North Amer. Med. and Surg. Jour. April* 1830.

Transactions of the Medical Society of the State of Tennessee, for the year 1830.

Through the politeness of the Corresponding Secretary, Dr. L. P. Yandell, we have been favoured with a copy of the "Transactions of the Medical Society of the State of Tennessee," which was incorporated by the late legislature of that state, and met in Nashville in May last, for the purpose of regulating the practice of physick in that state; and adopting regulations for the government and intercourse of its members. From this paper we take the liberty of furnishing our readers with copious extracts, in the hope that other legislatures and physicians may be induced by the example of those of Tennessee to "go and do likewise." It is, indeed, a matter of mortification and astonishment that in some of the oldest and most intelligent states of the Union, no legislative enactments have been passed on this important matter; and especially that in Pennsylvania and Kentucky—states which pride themselves on their Schools of Medicine—the door to practice is thrown widely open to every pretender, and under the very portals of the temple of Medical Science, cunning, impudence and quackery are freely admitted to prey upon the vitals of society. We earnestly pray that a spirit of reformation on this head will be aroused throughout the land, and that all those states which have not yet done so, will speedily adopt such measures in relation to the medical profession, as will prevent those from practice who cannot offer satisfactory evidence of the requisite qualifications.

Act of Incorporation.—An Act to incorporate a Medical Society in the State of Tennessee.—As health is universally acknowledged to be essentially necessary to the happiness and prosperity of society, and its preservation and recovery are essentially connected with an intimate acquaintance with the animal economy, and the properties and effects of Medicines, and as institutions formed on liberal principles, cherished by an intelligent community, and patronized by law, are eminently calculated to encourage the propagation and dissemination of such knowledge—therefore,

Sec. 1. *Be it enacted by the General Assembly of the State of Ten-*

nessee, That there shall be appointed by a resolution of this General Assembly, a sufficient number of learned and intelligent practising physicians in this state, who, when so appointed, are hereby constituted and formed into a body politic and corporate, by the name of the "Medical Society of the State of Tennessee," and they, and such other persons as said board may hereafter elect, in the manner prescribed by this act, and their successors, shall continue to be a body politic and corporate until the year 1860.

SEC. 2. *Be it enacted*, That the members of the Society may from time to time elect a President, Vice President, and Secretary, and such other officers as they may think necessary and convenient, and the members of said Society shall have power to determine the respective duties of the several officers, the length of time they shall hold their respective offices, also to authorize their President, or some other person, to administer such oaths to those officers as the Society shall think proper, not repugnant to the laws of this State or of the United States, and shall have a common seal, and power to alter and renew the same at their pleasure.

SEC. 3. *Be it enacted*, That the members of said Society may sue and be sued in all actions, real, personal, and mixed, and prosecute and defend the same to final judgment and execution, by the name and style of the "Medical Society of Tennessee."

SEC. 4. *Be it enacted*, That said Society may elect, under such regulations as it may adopt, such persons as may be thought worthy practitioners of Medicine and Surgery, to be members thereof; and such person or persons, so elected, shall be entitled to all the privileges and immunities of said Society, provided that he or they shall, within one year from the time of his election, sign the by-laws and regulations of said Society, or otherwise in writing signify his or their assent thereto; and shall have power to expel any of its members for misconduct.

SEC. 5. *Be it enacted*, That said Society shall have power and authority to enact laws and regulations, for the government of the same, as are not repugnant to the constitution and laws of this State, or of the United States, and to annex reasonable fines and penalties, not exceeding fifty dollars, to be sued for and recovered by said Society, for their use and benefit, in any court of law in this State, and also to fix the times and places of the meetings of said Society, which shall be at least once in each year, and may also determine the number of members necessary to constitute a quorum to transact business.

SEC. 6. *Be it enacted*, That the first meeting of said Society shall be held in the town of Nashville, in this State, on the first Monday in May 1830.

SEC. 7. *Be it enacted*, That the "Medical Society of Tennessee," so soon as they may meet and organize themselves, shall proceed to elect seven persons who reside in West Tennessee, not more than two of whom reside in the same county, who shall constitute a board of "Censors for Middle Tennessee:" and also in the like manner elect a board of "Censors for East Tennessee and the Western District."

SEC 8. *Be it enacted*, That the persons thus elected to constitute said boards of Censors shall hold their appointment for the term of one year from the time of their election, and until others shall have been elected to supply their places; they shall meet at such times and places as they may agree upon, at least twice in each year, and when so convened they shall proceed, under such regulations as shall be adopted by the Society, to examine any person or persons who may present themselves for such examination, touching their skill in the practice of Medicine and Surgery; and if on such examination the board of Censors shall deem such candidate sufficiently skilled in the science and practice of Medicine and Surgery, they shall grant to such candidate a license to practice the same in the State of Tennessee.

SEC. 9. *Be it enacted*, That no qualification shall be deemed necessary to entitle a candidate to examination, except he shall be twenty-one years of age, and of good moral character.

SEC. 10. *Be it enacted*, That if either of the boards of Censors shall obstinately refuse to examine any candidate having the above qualifications, each member who shall be present at the time of such refusal, and shall concur therein, shall be fined in a sum not exceeding five hundred dollars, to be sued for and recovered by such person or persons, so refused, in any court of law in this State, for his own use and benefit.

SEC. 11. *Be it enacted*, That each person who shall be examined by either board of Censors, shall pay for a license the sum of ten dollars, to be applied for the use and benefit of said Society.

SEC. 12. *Be it enacted*, That the Medical Society of Tennessee shall have power, at any regular meeting, to levy such contributions on its members as shall be thought necessary to effect any of the purposes of its establishment, and shall have power and authority to collect the same in any court of law in this State.

SEC. 13. *Be it enacted,* That any one of the Censors appointed under the provisions of this act shall have power and authority to grant a temporary license to any person or persons, to practice Medicine and Surgery, until a regular meeting of said board of Censors; after which time the said temporary license shall be void.

SEC. 14. *Be it enacted,* That said corporation shall at all times be subject to such rules, regulations and restrictions as may be thought necessary, and imposed by the General Assembly of this State.

EPHRAIM H. FOSTER,
Speaker of the House of Representatives.
JOEL WALKER,
Speaker of the Senate.

Passed, January 9th, 1830.

Medical Society.—The following gentlemen compose the Medical Society incorporated at the late session of the Legislature.

For Carter County.—Joseph Powell and William Mitchell.
Sullivan.—Elkanah R. Dulaney, Wm. R. Dulaney and Wm. Hammer.
Washington.—Sam'l. B. Cunningham and Daniel Kinney.
Greene.—Francis A. M'Corcle and Alex. M'Kenny.
Jefferson.—Joseph B. M. Reese, Wm. H. Deaderick, Henry Helm and William Moore.
Hawkins.—Hugh A. M. Walker and Archibald M'Kinny.
Grainger.—John W. Lyde, Wm. E. Cocke and P. B. Cobb.
Cocke.—George M. Porter and Dan'l. C. Chamberlain.
Sevier.—John W. Porter and David Bricker.
Claiborne————Shackelford and Jonathan Thacher.
Knox.—Joseph C. Strong, James King, Donald M'Intosh, William J. Baker and James G. M. Ramsey.
Anderson.—Hugh Crozier.
Blount.—Samuel Pride and John Temple.
Monroe.—F. H. Gregory, William J Morrison and Nathan Harris.
Roane.—Thomas A. Anderson and Lewis W. Jorden.
McMinn.—Horace Hicock and Joseph Alexander.
Rhea.—Gideon B. Thompson and Azariah Shelton.
Bledsoe.—Samuel L. Story and Henry B. Oden.
Marion.—Pleasant H. Butler and Samuel B. Mead.
Overton.—Nathaniel Coffee, and B. J. Bledsoe.
Jackson.—Wm. R. Kenner, Absalom John, and Nathan Montgomery.

White.—Madison Fisk and Robert Cox.

Warren.—Archibald Rogers and M. Hill.

Franklin.—Wallace Estill, Robert Turner and Jas. Montgomery.

Wilson.—James Frazier, Thomas Hunt, Sam'l. C. McWhirter, and L. W. White.

Smith.—John H. Ray, James A. White, ——— Dobson, and Don C. Dixon.

Rutherford.—James Maney, Wm. R. Rucker, Lunsford P. Yandell, F. E. Becton and Samuel Watkins.

Williamson.—Wm. G. Dickinson, Edward Breathitt, F. Stith, and William S. Webb.

Maury.—George W. Campbell, Thomas Brown, D. N. Sansom, H. E. Turner, John B. Hayes, John M'Gempsey and S. R. Porter.

Davidson.—Samuel Hogg, James Roane, Felix Robertson, R. A. Higginbotham, Boyd M'Nairy, James M. Walker, Charles Pugsley, John Waters, John Maxey and James Overton.

Sumner.—Elmore Douglass, James A. Blackmore, Wm. T. Hodge, Allen Gillespie and David M. Porter.

Roberston.—Archibald Thomas, Christian Allen and T. J. Watson.

Montgomery.—George B. Hopson, Thomas T. Watson, Lewis W. King and J. H. Marable.

Bedford.—John H. Kain, M. D. Mitchell, and Preston Frazer.

Giles.—William Topp and L. Cooper.

Stewart.—Christopher Clements.

Lincoln.—William F. Smith, Allen C. Gillespie, E. H. Hickman and William Bonner.

Hickman.—William D. Gowen, James G. Smith, and Joel K. Walker.

Dickson.—Belfield N. Carter, and Henry Marable.

Carroll.—Robert Nicholson, James Henderson, and Seth W. Bell.

Gibson.—John H. Crisp, and William W. Lea.

Dyer.—Joseph Degraffenreid, and Edward Burchet.

Obion.—William Wilkerson.

Henry.—Thomas K. Porter, Francis Reid and E. R. Anderson.

Weakley.—Pulaski P. Bell, and Thomas Edwards.

Madison.—James Young, Robert Fenner, and ——— Loving.

Henderson.—John A. Wilson and ——— McKissack.

Haywood.—Allen J. Barbee.

McNairy.— ——— Young.

Hardeman.—Daniel M. Guinn, and David Brown.

Fayette.— ——— Williams, and ——— Higgason.
Shelby.— ——— Russell and ——— Christian.
Lawrence.—Gabriel Bumpass.
Wayne—Joel Hancock.
Hardin— ——— Watt.
Tipton—J. A. Greene and Charles G. Fesbee,
Perry.—Daniel L. Sanders.
Humphreys.—F. A. Dallam.

Transactions of the Medical Society of Tennessee.—The association of Physicians incorporated by an act of the Legislature of Tennessee, at its last session, assembled in Nashville on Monday, the 3d of May.

The meeting, although not a full one, was sufficiently numerous for the transaction of business.

A Committee was appointed to prepare and report a code of *By-Laws*, for the government and regulation of the Board. Drs. ALEX. M'KINNEY, of East Tennessee, BOYD M'NAIRY, WALLACE ESTILL, F. STITH and JOHN B. HAYS, of Middle Tennessee, and JAMES YOUNG, of the Western District, composed this Committee.

By-Laws of the Medical Society of Tennessee.

ART. 1. The Society shall convene annually on the first Monday in May, at Nashville.

ART. 2 The officers shall consist of a President, Vice President, a Recording Secretary, Corresponding Secretary and a Treasurer.

ART. 3. Twenty-five members shall constitute a quorum to transact business.

ART. 4. The officers shall be chosen by ballot, and continue in office for the term of two years; and, in case of a tie, the presiding officer shall give the casting vote—and the majority shall elect.

ART. 5. The President shall preside at all meetings, preserve order and regulate the debates; state and put questions agreeably to the sense and intention of the members; he shall have power to fine any member for disorderly conduct, in a sum not exceeding five dollars for each offence; he may, with the concurrence of the Vice President, call a general meeting of the members, at any time intervening the periods fixed for the established meetings, whenever he may think that the interests of the Society shall require a special meeting—of which one

month's notice shall be given in some one of the newspapers of Knoxville, Nashville and Jackson.

ART. 6. In the absence of the President, his duties shall devolve on the Vice President; and, if neither of them be present, a presiding officer *pro tem.* shall be appointed, who shall perform the duties of President.

ART. 7. It shall be the duty of the Recording Secretary to attend each meeting of the Society, call over the names of the members, and take down the minutes, which shall be read at the next meeting; he shall keep a list of the members admitted; also, a list of the names and residences of all those persons who shall have received a license from the Boards of Censors.

ART. 8. It shall be the duty of the Corresponding Secretary to write and answer all letters in behalf of the Society; to manage all matters of correspondence, and to make a faithful record of such communications as this Society may think worthy of being preserved.

ART. 9. The Treasurer shall receive all donations and collect all the contributions arising from such laws and regulations as the Society shall from time to time make; he shall likewise keep all the monies and securities belonging to the Society; and shall pay all orders signed by the President, Vice President, or presiding officer for the time being —which orders shall be his vouchers for his expenditures.

Admission of Members.—ART. 10. Any medical gentleman who shall be proposed as a member of this society, may be admitted by the vote of two thirds of the members present. On admission, he shall sign the Constitution and By-laws, and pay into the hands of the Treasurer such sums as may annually be paid by the members of the Society.

ART. 11. No person residing within the limits of the State shall be admitted as an Honorary Member; but any medical gentleman, residing without the limits of the State, having been proposed, and satisfactory reasons having been given why he should be admitted, may be elected by the vote of two thirds of the members present.

Censors.—ART. 12. The Medical Boards of Censors shall be chosen by ballot.

ART. 13. It shall be the duty of each of the Boards of Censors to subject the candidates for licences to an examination on the following branches of the profession, viz. Anatomy, Physiology, Pathology, Chemistry, Materia Medica, the Practice of Medicine, Surgery and Midwifery; except when the candidate presents such testimonials of

qualifications as, in the judgment of the Board, may preclude the necessity of examination.

Art. 14. Not less than five Censors shall constitute a quorum of either Board. If five only are present, one vote may reject a candidate; if more are present, two negative votes are required to reject.

Art. 15. The Dean of each Board of Censors shall keep a record of its proceedings, and from time to time transmit the same, with the fees by him received, to the Recording Secretary.

Art. 16. The Licentiate shall be furnished by the Censors with a license, printed on parchment, signed by themselves, the President and Recording Secretary, and stamped with the seal of the Society, after the following manner:

"We the subscribers, Censors of the Tennessee Medical Society, duly appointed and authorized, have examined H. B. of C. in the county of D. a candidate for the practice of Physic and Surgery, and having found him qualified, do approve and license him as a Practitioner in Medicine, agreeably to the law in that case made and provided."

Art. 17. The President shall have authority to fill up any and every vacancy that may occur between the regular meetings of the Society, by the concurrence of the members of the Board in which such vacancy or vacancies may occur.

Art. 18. The President or presiding officer shall, at each annual meeting, appoint an Orator, whose duty it shall be to deliver a suitable address, at the next annual meeting, on some subject appertaining to the science of Medicine, Natural History or Botany, under a penalty of twenty-five dollars.

Art. 19. On motion, at any regular meeting, a member may be expelled by a vote of two thirds of the members present; previous notice, with specification of the charges, having been given him by his *accuser* or *accusers*, three months before the meeting at which such motion shall be made: provided, nevertheless, that where any charge shall be made by one member of the Society against another, the *accused* shall be at liberty to call upon any *five* members of said Society, most convenient to the parties, who shall, on reasonable notice, examine into said charge or charges, and, if deemed of sufficient importance, shall report the same to the next meeting of the Society; otherwise, finally dispose of the same in such manner as they may deem just and proper.

Art. 20. The *form of oath* administered by the presiding officer of the Society, or any Judge of the State, shall be in the following words:

"I, A. B. of the county of C, and State of Tennessee, do hereby solemnly affirm that I will faithfully, truly and impartially perform the duties assigned me, as prescribed by the act of the Legislature, and the By-laws of this Society, to the best of my knowledge and abilities."

ART 21 No one of the foregoing By-laws shall be altered or amended without the concurrence of two-thirds of the members present.

The Society then proceeded to an election of officers. Whereupon the following gentlemen were found to have received a majority of votes, and were declared by the Chairman duly elected.

Dr. JAMES ROANE, of Nashville, President.

Dr. JAMES KING, of Knoxville, Vice President.

Dr JAMES M. WALKER, of Nashville, Recording Secretary.

Dr. LUNSFORD P. YANDELL, of Rutherford, Corresponding Secretary.

Dr. BOYD M'NAIRY, of Nashville, Treasurer.

Professor CALDWELL (of Transylvania University) being present by invitation, was on motion of Dr. BECTON, of Murfreesborough, unanimously elected an Honorary Member of the Society.

The following gentlemen were then elected *Censors.*

For the Western District.

Doctor JAMES YOUNG, of Jackson, Madison county.
ROBERT NICHOLSON, of Huntingdon, Carrol county.
E. R. HENDERSON, of Paris, Henry county.
ALLEN J. BARBEE, of Brownsville, Haywood county.
JOHN A. WILSON, of Henderson county.
F. T. REID, of Paris, Henry county.
JOHN H. CRISP, of Gibson county.

Middle Tennessee.

Doctor JAMES MANRY, of Murfreesborough, Rutherford county.
FERDINAND STITH, of Franklin, Williamson county.
JOHN B. HAYS, of Columbia, Maury county.
WALLACE ESTILL, of Winchester, Franklin county.
JOHN H. MARABLE, of Montgomery county.
ELMORE DOUGLASS, of Gallatin, Sumner county.
SAMUEL HOGG, of Nashville, Davidson county

East Tennessee.

Doctor Wm. H. Deaderick, of Jefferson county.
Alex. F. M'Kinny, of Greenville, Green county.
John W. Lyde, of Grainger county.
Donald M'Intosh, of Knoxville.
John Temple, of Blount county.
Joseph C. Strong, of Knoxville.
Thomas A. Anderson, of Roane county.

Doctors Becton, Yandell, F. Robertson, Hays and Kain were appointed a committee to prepare and present a code of Medical Ethics for the government of the members of this Society, in their professional intercourse.

In the afternoon of the same day the committee reported the following, (it being the code enacted by the "Central Medical Society of Georgia," at their session in 1828) which was adopted by the Society.

Medical Ethics.

"1. It is the duty of every medical practitioner to treat his patients with steadiness, tenderness and humanity, and to make due allowances for that mental weakness which usually accompanies bodily disease. Secresy and delicacy should be strictly observed in all cases in which they may seem to be peculiarly required.

"2. The strictest observance of temperance cannot be too strongly inculcated on the minds of the practitioners of medicine and surgery—a clear and vigorous intellect and a steady hand being absolutely necessary to the successful practice of those branches of medical science.

"3. Unfavourable prognostications should never be made in the presence of patients; yet, should there seem to be immediate danger, it becomes the duty of the medical attendant to apprise the patient's friends of that circumstance.

"4. In every instance in which one physician has been called on to visit the patient of another, a consultation with the former medical attendant should be proposed. Consultations in difficult cases should always be recommended, and the physician called on for that purpose should always pay the greatest degree of respect to the practitioner first employed, and allow *him* the privilege of delivering all the directions agreed upon.

"5. Special consultations are sometimes wished for; in such cases the physician called on should carefully guard against paying another

visit, unless he should be requested to continue his services by the patient, or some of his friends.

"6. When one physician is called on to visit the patient of another in his absence, or during short indisposition, he should not manifest a wish to continue in attendance any longer than the physician first called on should be able to resume the charge of the case, unless a continuance of his services should be expressly wished for by the patient or his friends.

"7. Physicians should not visit their patients too frequently, lest seeing them oftener than necessary might produce unsteadiness in the treatment.

"8. Theoretical discussions should not be too freely indulged in consultations, as they frequently give rise to much perplexity, without any improvement in practice.

"9. The junior physician in attendance should always deliver his opinion first, the others according to seniority, and a majority should decide; but, in the event of a tie, the physician first in attendance should give the casting vote in regard to the future treatment, and to him should be entrusted the future management of the case, unless the patient or his relations should object to his being continued.

"10. Although the possession of a diploma, honorably acquired, furnishes presumptive evidence of professional ability, and entitles its possessor to pre-eminence in the profession, yet, the want of it should not exclude practitioners of experience and sound judgment from the fellowship and respect of the regular graduate.

"11. In consultations punctuality in meeting at the same time should be strictly observed, but the physician who first arrives should wait a reasonable length of time for the arrival of others. A minute examination of the patient, however, should not take place until one or more of the medical attendants are present, except in cases of emergency. All subsequent visits should, if practicable, be made by mutual agreement, and no medical discussion should take place in the presence of the patient.

"12. Attendance on the members of the profession or their families should always be gratuitous, but should not be officiously obtruded. Should the circumstances of the medical practitioner indisposed well enable him to make a recompense for medical services rendered to himself, his wife or family, it is his duty to do so, especially if he reside at a distance.

"13. When one medical practitioner is called on to visit a patient whose recovery has been despaired of by the physician first in attendance, and the disease should afterwards terminate fatally under his management, he should avoid insinuating to the friends of the deceased, that if he had been called on a day or a few hours sooner he could have effected a cure. Such a course of conduct is highly reprehensible, and empirical in the extreme. And in the event of the patient's recovery, such a person should not assume all the credit, as the cure might have been partly effected by the medicines prescribed before he took charge of the case.

"14. The use of nostrums and quack medicines should be discouraged by the faculty, as degrading to the profession, injurious to health, and often destructive of life. Should patients labouring under chronic complaints obstinately determine to have recourse to them, a reasonable degree of indulgence should be allowed to their credulity by the physician; but it is his sacred duty to warn them of the fallacy of their expectations, and the danger of the experiment, and the necessity of strict attention to the effect produced by them, in order that their bad effects, if any, should be timely obviated.

"15 No physician should either, by precept or example, contribute to the circulation of a secret nostrum, whether it be his own invention and exclusive property, or that of another. For, if it be of real value, its concealment is inconsistent with beneficence and professional liberality; and if mystery alone give it value and importance, such craft implies either disgraceful ignorance or fraudulent avarice.

"16. A wealthy physician, or one retired from practice, should refuse to give gratuitous advice, unless the danger of the case (in the absence of the practising physician) or the poverty of the patient should warrant him in so doing. In all cases where he may be preferred, he should recommend a consultation with some one engaged in active practice. This rule should be strictly observed, as a contrary course is gratuitously depriving active industry of its proper reward.

"17. When a physician is called on suddenly to visit the patient of another, in consequence of some unexpected or alarming change in the symptoms, he should adopt a temporary plan of treatment, suited to present circumstances. He is not warranted in interfering afterwards unless requested to take charge of the case, when he should propose an immediate consultation with the physician previously employed.

"18. Physicians should never neglect an opportunity of fortifying

and promoting the good resolution of patients suffering under the bad effects of intemperate lives and vicious conduct; and in order that their counsels and remonstrances may have their due weight, it will readily be seen that they should have full claim to the blameless life and high moral character which we have stated to be a necessary pre-requisite to an honorable stand in the profession."

The same Committee was continued, and instructed by the Society to report again at the next anniversary meeting.

Resolved, That it is the deliberate opinion of this Society that ardent spirits are rarely useful as a medicine, and that their habitual use is productive of the most lamentable consequences to the human frame —such as derangements of the vital organs, causing a short and miserable existence, and still more miserable death:—and, therefore,

Resolved, That this Society urgently recommend to their fellow-cit zens a total abstinence from ardent spirits, except in cases prescribed by physicians.

Resolved, also, That each member of this Society be requested to report, at their next annual meeting, the number of those who shall die from *Intemperance* during the next year, and also the number of those diseased from the same cause.

Resolved, That until the next annual meeting the different Boards of Censors may issue licenses on common paper to such applicants as may be deemed worthy to practise Medicine and Surgery.

Resolved, That the President appoint three individuals of the Society to read Essays at the next annual meeting, on the Medical Topography and the Diseases of their respective counties.

Dr. James Ramsey, of E. Tennessee, Dr. Becton, from Middle Tennessee, and Dr. James Young, from the Western District, were appointed by the President, with the concurrence of the Society.

The Society proceeded to the election of new members; whereupon the following gentlemen were elected, who, according to the Constitution, will become members by signing that instrument, or by signifying their acceptance to the Recording Secretary, and the payment of one dollar, their yearly contribution.

Davidson County—Thomas Jennings, John Irwin, Peyton Robertson, Gerard Troost, John Newman, Adam Goodlet, John O. Wharton, T. B. Dudley, Patrick Neilson, Dr. Lawrence, Dr. Bigelow, Dr. S. Nye, Jas. W. Hoggatt, Wm. Gwin, John L. Hadley, Sen'r, George Keeling, Dr. Patterson & J. C. Hicks.

Sumner County.—F. L. Sharpe, John J. Franklin, Joel H. Green, Baker Walsh, Evans Mabray, Dr. Gray, Dr. Mentlo and Joseph M. Sittler.

Maury County.—Robt. C. K. Martin, J. W. S. Frierson, S. Jourdan, S. Caldwell, St. Clair Caldwell, Dr. M'Kitchen, Dr. Konky, A. H. Buchanan, Thos. Kennedy, Dr. Briscoe, J. T. Harris, Dr. Greenfield and J. B. Sanders.

Giles County.—Wm. H. Tarpley, E. Field, E. Eldridge, George A. Glover, Dr. Harris, Dr. Herald, Dr. Philips, Dr. Butler and Dr. Anderson.

Rutherford County.—Alfred Hartwell, George Thompson, Jno. R. Wilson, H. H. Treadway, Wm. D. Gowen, Henry Holmes, P. H. Mitchell, Wm. M. Yandell, Dr. Gooch, Wm. L. Thompson, Thos. H. Read, and Laf. Ezell.

Williamson County.—Samuel Crockett, H. B. Ewing, R. H. Gentry, L. O. Bryan, J. M. Watson, Elijah Thompson, William M'Gee, B. Hughes, Milton Ewing, Felix Ewing, Doctor Hancock, Doctor Jourdan and John L. Hadley, Junior.

Bedford County.—A. B. Morton, Dr. Baskett, Dr. Norton, Dr. Robinson, Dr. Gaither, Charles Sutton and John L. Armstrong.

Lincoln County.—Dr. Barksdale, Dr. Anderson, John V. M'Kinney, Charles M'Kinney and Robert Stone.

Hickman County.—Dr. M'Carrol, Dr. Smith and C. B. Thompson.

Madison County.—M. B. Cook, Dr. Goodwin, George Snider, S. W. Vaughn, Erasmus D. Fenner.

Blount County.—Alexander M'Gee, Jas. F. Gillespie.

Knox County.—Leonidas Baker, James Morrow and Freeman Hurdle.

Weakley County.—John Travis.

Wilson County.—F. Crutchfield.

Greene County.—Dr. Williams.

McMinn County.—H. P. Fore, Jesse Edington and Samuel F. Jordan.

Grainger County.—Jas. R. Cocke.

Hawkins County.—William Walker and Dr. Cook.

Sullivan County.—John Moore, John Hoyal, Dr. Broyles and James Woods.

Washington County.—James Rankin.

Jefferson County.—John Grigsby.

Smith County.—Sidney Smith.
Tipton County.—Dr. Thompson and Dr. Johnson.
Fayette County.—Isaac N. Jones.
Haywood County.—J. C. Jones.
Henderson County.—Dr. Greer.
Franklin County.—William Estill.
Dyer County.—Clement G. Jamison.

JAMES ROANE, *President.*

J. M. WALKER, *Rec. Secretary.*

Prize Essay.—The Medical and Chirurgical Faculty of Maryland, at their annual convention held in the city of Baltimore, on the 7th and 8th June, 1830, passed the following resolution, viz.

Resolved, That a committee of seven be appointed to award a premium of one hundred dollars for such essay as they, or a majority of them, shall consider worthy thereof. The subject of such essay to be selected by said committee.

In conformity with the benevolent intentions of the Faculty, expressed in the aforesaid resolution, the committee offer a premium of $100 for an essay upon the nature and sources of the Malaria or noxious Miasma, from which originate the family of diseases usually known by the denomination of bilious diseases; together with the best means of preventing the formation of malaria, removing the sources, and obviating their effects upon the human constitution when the cause cannot be removed.

The committee have been induced to call the attention of the profession to this subject, because of its vast importance to society at large. The immense extent to which this fruitful cause of disease operates, has not yet been accurately calculated, nor any probable estimate made of the mortality which it occasions. The public attention has been justly directed to other subjects of general improvement, but we believe no adequate effort has yet been made to awaken and direct the public mind to the prevention of the evils dependent upon Malaria, although it is well known to medical men to be extending its influence, and threatening to depopulate some of the finest sections of this country, as it has already depopulated some of the fairest portions of the old world.

Candidates for the prize are to cause their dissertations to be delivered to the subscriber, in Baltimore, (postage paid,) on or before the

first day of May, 1831. Each dissertation to be accompanied with a sealed letter, superscribed with a motto corresponding with that prefixed to the essay. None of the letters, except that to which the motto of the successful essay shall be affixed, will be opened; the other essays shall be disposed of according to the direction of the proprietors.

HENRY W. BAXLEY,
Secretary to the Committee.

Vaccine Institution.—The "*Medical and Chirurgical Faculty of Maryland,*" at their annual convention, held in Baltimore on the 7th instant, having established a "Vaccine institution," and appointed the subscriber agent, he respectfully notifies the medical public, that he will keep a constant supply of *fresh genuine vaccine matter*, which may be had by application in person, or in writing, at all seasons of the year and at an hour's notice.—Price for each vaccine *scab or crust*, two dollars, or by remitting five dollars (post paid), three crusts will be furnished.—All orders will be attended to with promptness and despatch, directed to

WILLIAM ZOLLICKOFFER, M. D.
BALTIMORE, *June 12th*, 1830. *Vaccine Agent.*

Public Vaccine Institution of Philadelphia.—The Select and Common Councils of the City of Philadelphia, having so altered the ordinance providing for the gratuitous vaccination of the poor of said city, as to make it the duty of four individuals; and the subscribers having been appointed to the performance of the above duties, would respectfully inform the Medical Profession throughout the United States, that it is their intention to keep a constant supply of genuine fresh vaccine virus, which they will supply on receiving a small remuneration for the extra trouble it will occasion. Applications, (post paid) enclosing three dollars, addressed to either of the subscribers, will be promptly attended to.

DAVID C. SKERRETT, M. D. GEO. SPACKMAN, M. D.
No. 155, South Tenth street. *Twelfth street, one door below Race.*
JOSEPH PANCOAST, M. D. AMOS PENNEBAKER,
No. 103, Walnut Street. *No. 91, North Fourth street.*

TRANSYLVANIA UNIVERSITY.

MEDICAL DEPARTMENT.

—

THE Course of Lectures in this institution will commence, as usual, on the 1st Monday in November, and terminate on the 1st Saturday in March. The arrangement for the ensuing session will be as follows, viz.

Materia Medica and Medical Botany, by Dr. SHORT, daily, at 9 o'clock A. M. Ticket to the course $15.

Obstetrics and the Diseases of Women and Children, by Dr. RICHARDSON, daily, at 10 o'clock, A. M., $15.

Institutes of Medicine and Clinical Practice, by Dr CALDWELL, daily, at 11 o'clock, A. M. $15

Anatomy, $15. } By Dr. DUDLEY, ten times a week, viz. daily at
Surgery, $15 }
12 o'clock, and four times a week at 3 o'clock, P. M.

Chemistry and Pharmacy, by Dr. BLYTHE, daily at 1 o'clock, P. M. $15.

Theory and Practice of Physick, by Dr. COOKE, daily at 4 o'clock, P. M. $15.

The Matriculation fee is $5, and that of Graduation $20.

Two full courses of Lectures are required by the laws of the Institution, before a student is permitted to present himself for examination, as a candidate for the degree of Doctor of Medicine, unless he shall have attended one full course in some other respectable school, or shall have practised Medicine creditably for the term of four years: in either of which cases one entire course of Lectures will entitle the candidate to examination. The examination of candidates for degress, and the commencement for conferring them, are held immediately after the close of each session.

After attending two full courses of Lectures, or after having graduated in this Institution, the pupil has the privilege of attending all the lectures of the different Professors gratuitously, for any length of time.

The Medical Library now contains upwards of *three thousand vol-*

umes, comprising all the rare and standard works of Medicine and the collateral branches of science; and is still constantly increasing by the addition of many of the periodical publications of Europe, and those of America; as well as most of the recent works on the profession, as they appear either at home or abroad. In addition to these, arrangements have been made whereby a large additional number of TEXT BOOKS to the different courses are laid before the student, so that the inconvenience heretofore felt on that score will be removed. To this extensive collection the Medical Students have free access, and are privileged to take out two volumes at a time. The Library is opened as a reading room every day, and until 10 o'clock every night: during the winter, it is comfortably warmed and lighted.

"The Lexington Medical Society," holds regular meetings every week throughout the winter, and monthly during the summer. At these, communications are read by the different members on subjects appertaining to Medical Science, and free discussions are elicited by them. The Medical Professors occasionally attend, read papers, and partake in the debates.

An extensive Lunatic Asylum, situated within the limits of the town, affords at all times a variety of interesting cases, presenting every modification of mental derangement. Of this institution the Medical Professors are, *ex officio*, the attendants and consulting physicians; and to the practice of the house all Students of Medicine have free access.

Boarding may be had in genteel houses for $2. 50, to $3. 00 per week —this includes all incidental charges for washing, fuel, candles, &c.

Those who travel to Lexington on horse-back and may wish to retain their horses to return, can have them kept in the best manner, at the livery stables of the town, for $5. 00 per month; and in the neighbourhood for $4. 00.

To meet these expenses students are advised to provide themselves with United States paper, or specie; the notes of many of the local Banks, although at par in their neighbourhoods, being at some discount here.

By order of the Faculty.

C. W. SHORT, M. D. DEAN.

Lexington, Kentucky, August, 1st 1830.

THE

TRANSYLVANIA JOURNAL

OF

MEDICINE

AND THE ASSOCIATE SCIENCES.

NOVEMBER, 1830.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Thoughts on Febrile Miasms, intended as an answer to the Boylston medical prize question, for the year* 1830, "*Whether Fever is produced by the decomposition of animal or vegetable substances, and if by both, their comparative influence?*" By CHARLES CALDWELL M. D.

Strenue, sed modeste conandum est.

[Extract of an official letter from Dr. Hayward, of Boston, to Professor Caldwell of Lexington, Kentucky.]

BOSTON, *August 5th*, 1830.

DEAR SIR,

It gives me great pleasure to state that the Boylston Committee on Prize Questions, at their annual meeting held yesterday, unanimously awarded to you their prize of fifty dollars, or a gold medal of that value, for your Dissertation on the question, "Whether Fever is produced by the decomposition of animal or vegetable substances; and if by both, their comparative influence?" with the motto, "*Strenue, sed modeste conandum est.*"

The Committee desired me to say, that though they felt bound by

precedent not to ask any successful author to publish his Dissertation, it would still give them great pleasure to see yours in print.

GEORGE HAYWARD,
Secretary of the Boylston
Committee on Prize Questions.

CHARLES CALDWELL, M. D.

As the importance of the subject of this memoir is as universally known and acknowledged, as that of any belonging to the science of medicine, it would be superfluous to consume time, at present, in illustrating that of which no one is ignorant, or in exhibiting proof of what no body doubts. To say nothing of the evidence to that effect, deducible from its having been made the subject of a "prize question," by the corporation of an ancient and distinguished university, its importance is fearfully attested by the fact, that it is the source of not only the most common, but the most extensive and formidable calamities, that befall the human family. While war and famine, inundations and earthquakes destroy their thousands, febrile miasms bring millions to the grave. And the amount of suffering which they produce, in a given number of fatal cases, surpasses, not a little, that of death from most other causes. Even when death does not occur, the effects of fever are often more painful and grievous than those of mortal hurts from mechanical violence.

Could this memoir, then, be rendered worthy of its subject, its value would be incalculable. An essay throwing competent light on the true causes of fever, as respects their origin, nature, effects, and collateral relations, would confer higher benefits on man, and redound more to the glory of its author, than any other discovery in medicine. Even that of the circulation of the blood would not equal it in either brilliancy or usefulness. Nor would it be comparable to it in the extent and multiplicity of its scientific connexions. By disclosing the proper modes of both prophylaxis and removal, the discovery alluded to, should it ever be made, will either prevent fever entirely, or teach the measures requisite to destroy it in its birth. In either case, old

age and accident would become, immeasurably more than they ever have been, the chief outlets of human life.

But such a memoir must not be looked for. To say nothing of the writer's incompetency to produce it, success in the attempt is inhibited to every one, by obstacles arising out of the present condition of medical science. Facts are wanting to inform us of all that it concerns us to know of febrile poisons. The chief object that will be aimed at, therefore, in this article, will be to collect such scattered lights as are faintly glimmering from various quarters, and endeavour, by embodying them, and offering a few comments on them, to augment their lustre.

Nor is it my intention to treat of all the miasms productive of fever, with the fulness requisite even to such an elucidation of them as might be presented. An effort like this would compel me to traverse a field of enquiry by far too extensive, as well as too multifarious in its productions, to be compassed and duly explored, within the limits to which I must confine myself. My purpose is to attempt a free examination of only one of the most important of them; and on that I shall enter, after a few further preparatory remarks.

Febrile miasms are of two kinds, *contagious* and *infectious*. The contagious are the product of living matter. They are generated by morbid secretory action, in some part of the human system, when labouring under disease. A contagious miasm, which may be called febrile, is also secreted by the salivary or mucous glands of the rabid dog. The infectious are produced by changes in dead matter, presumed to be of a chemical character. But the evidence to this effect is *only* presumptive. It is not known by what peculiar action nature forms those infections; nor are they the offspring of any process that the chemist can institute. In no laboratory but that of nature, have they yet been generated. All that can be safely asserted of their origin is, that they are the result of some mutation in dead matter, produced, most probably, by the united influence of the two processes of decomposition and recomposition.

I have said that contagious miasms are generated by morbid

secretory action, in living matter. But they are not always thus generated. They sometimes derive their origin from dead matter, and then attacking the human system, propagate their kind, by the peculiar action which they there induce. This is the case with small-pox, when it prevails as an *epidemic*, as it lately did in Philadelphia, and other parts of the United States. Under these circumstances it becomes an atmospherical disease, and often attacks individuals who have not been exposed to contagion from the sick; who have not even been in the neighbourhood of those affected by it, nor near to any other local source of the complaint. As the atmosphere itself is contaminated, it cannot be imagined that the poison which loads it is the product of secretion. Whence it comes we know not. But we must not, on that account, refer it to an inadequate source. The cause of epidemic small-pox, moreover, is as well known to us as that of any other epidemic. We are ignorant of the origin of every complaint of the kind. But we know something of their laws and peculiarities, as distinguished from other diseases, and can usefully avail ourselves of this knowledge in practice.

When small-pox prevails epidemically, it is more malignant and fatal, than when it depends, for its propagation, on secreted contagion alone; and it often attacks those who have suffered from it before. The reason of this is obvious. The constitution of the atmosphere, subduing and modifying the human constitution, predisposes to the complaint; and the secreted virus produces it, by acting on the system in this weakened condition. It is thus that during the prevalence of a pestilential constitution of the atmosphere, miasm from local sources generates disease much more readily, and gives to it more malignity, than at any other time.

It may be said to constitute a part of our creed in medicine, that small-pox was introduced into Europe, by the soldiers of the Cross, on their return from Palestine. The opinion *may possibly* be true; but it is deficient in proof. It resembles that which derives Lues Venerea from the continent of America; and is probably no better founded. There is reason to suspect that

both hypotheses are the result of that principle of our nature, which induces us to refer new diseases to remote sources, and to vindicate our country, as well as ourselves, from the imputation of producing an evil of any kind. From this principle arose, among the physicians of the United States, the late and long continued controversy respecting the origin of yellow fever.

Its whole history would seem to proclaim, that when small-pox first occurred in Europe, it spread *as an epidemic.* On secreted *contagion alone* its propagation does not appear to have depended. Its progress was, by far, too rapid for that, and the sphere of its prevalence, in a given time, too extensive. It passed from city to city, and from country to country, like a general pestilence, carried by the atmosphere; not like an insulated disease, propagated, by a secreted poison, from the sick to the well. Nor did the crusaders first open the intercourse between Europe and Asia. By means of commerce, it had been open for ages. Why, then, had not small-pox been previously introduced? Had its introduction depended on the importation of contagion alone, it ought to have been effected at a much earlier period. But a suitable constitution of the atmosphere was wanting. That having occurred, in sufficient strength, contagion was not necessary; not necessary, I mean, to the mere commencement of the complaint; although it would subsequently contribute to its progress and perpetuation. From whence, it might be asked, did Asia derive small-pox? Or is there any thing, in that quarter of the globe, to generate it, more than in Europe? No pathologist will hazard his reputation, by giving to this latter question an affirmative reply. To the former we know the Asiatics replied, that the disease was a production of the interior of Africa. And to what source did the Africans attribute it? To this interrogatory no satisfactory answer can be rendered, for want of an acquaintance with the speculations and dogmas of those sable philosophers. We must not be surprised, however, at being shortly informed, by one of the numerous travellers in Africa, that the wise men of that continent import the complaint from some fabled region

still further to the south. In fact, the search, by physicians and others, after the origin of small-pox, is as fruitless as that of the poet after the north;

"Ask, where's the north?—at York, 'tis on the Tweed;
"In Scotland, at the Orcades; and there,
"At Greenland, Zembla, or the Lord knows where.

All things considered, it is highly probable, that, instead of being related to each other as cause and effect, the wars of the Cross, and the first visitation of Europe by the small-pox, were related only as contemporary events, each being in a different line of causation. This probability is the stronger from the consideration, that the disease had not existed in Asia very long before the period of the Crusades. Having been produced there, by a favourable constitution of the atmosphere, that constitution required some time to travel westward, cross the Hellespont, and carry the complaint along with it into Europe.

Were the present a suitable occasion to enter on the discussion, it might be made appear highly probable, if not certain, that the *primitive origin* of all contagious poisons must be referred to changes in dead matter. There would seem indeed to be no other source to which it can be referred. Such poisons must have had an existence before they could, by attacking the system of man, or any other animal, have reproduced their kind. And that existence must have been originally derived from some charge in dead matter.

To the class of contagious miasms productive of fever belong those of small-pox and kine-pox certainly, and *possibly* that of measles. I have used the word *possibly*, because I am far from being convinced, that measles are contagious. On the contrary, the arguments that may be adduced against such a belief appear to preponderate. I am aware that the charge of heterodoxy will be almost universally preferred against this sentiment. But such accusations never move me. To make the worst of it, heterodoxy means nothing more than a difference in opinion from the majority. And although the custom and constitutions of our country authorize us to settle, by numbers, all matters of a political nature, I know of no such authority, as relates to

science. Besides, I have not forgotten the time, when the charge of heresy was loudly and angrily urged against all who denied the contagion of yellow fever. Yet there is scarcely now in the United states, a physician of any eminence who believes in it. I repeat, then, that, for reasons which might be easily and abundantly adduced, were the occasion an expedient one, I am inclined to deny the contagion of measles.

On the contagious nature of chicken-pox I decline giving an opinion. As far as my knowledge extends, authentic facts are wanting to support one. But, that that disease always begins and spreads by a secreted contagion, cannot be maintained. That it generally, if not always, *commences* from an *infectious* influence, and is thus also communicated, in most instances, to those who have had no intercourse with the sick, is a position which will not, I think, be controverted. It rests on facts that are innumerable, and as well established as any connected with medical science.

To infectious miasms belong those of influenza, scarlatina, pertussis, typhus fever, peripneumonia typhoides, pestis vera, and the entire family of bilious affections, from yellow fever, to the mildest intermittent. The ephemera sudatoria of England was also infectious, not contagious. Of dengue the same may be affirmed.

Nor does the catalogue of infectious miasms terminate here. All endemic and epidemic diseases arise from poisons of this description; all, I mean, except small-pox, which, although generally propagated by contagion alone, appears at times, as already represented, in an epidemic form—But, that I may, throughout this discussion, be the more clearly and certainly understood, I perceive the necessity of giving, before proceeding further, definitions of a few terms, which I shall occasionally employ.

An endemic is a complaint of limited extent, arising from causes connected with localities. In some instances, it is connected also, more or less, with season. Thus goitre and cretinism are the perennial products of deep and unsunned val-

lies; while intermitting fever is the annual growth of low and level alluvial situations.

An epidemic is a disease whose range may be unlimited. It is the product of causes that have no necessary connexion with either time or place, and prevails, therefore, at all seasons, and in all situations. Of this description are influenza, measles, scarlatina, and peripneumonia typhoides. Although measles and scarlatina are often denominated vernal diseases, they appear and spread at all times of the year.

A constitution is a peculiar condition of the atmosphere, arising from causes concealed from the senses, but manifest in their effects, and producing complaints of a peculiar character. Thus one constitution gives rise to influenza, another to scarlatina, a third to intermittents, and a fourth to yellow fever. And during any given constitution, especially if it be strong, no febrile disease can prevail, except that which is congenial to it. Hence no two endemics or epidemics can coexist in the same place. As well might two portions of matter occupy at once the same point of space.

Endemic and epidemic constitutions of the atmosphere, then, are the product of infectious miasms. I repeat, therefore, that all febrile affections capable of assuming an endemic or epidemic form, must be regarded as infectious. This is as true of epidemic pleurisy, peripneumony, and catarrhal fever, which occasionally succeed autumnal remittents and yellow fever, as it is of those complaints themselves.

It is on this principle alone that we can explain satisfactorily the following phenomenon of disease; which uniformly presents itself, but has never, I think, received the consideration it deserves. The febrile complaints of winter are generally supposed to depend on the sensible qualities of the atmosphere. They are considered as the more immediate productions of its vicissitudes. Hence when the temperature of the weather sinks very suddenly from higher to lower, and disease follows, it is attributed merely to the taking of cold. Yet, under sensible qualities and vicissitudes precisely alike, very dissimilar diseases occur. One change of weather takes place, and an

epidemic pleurisy or peripneumony appears. At another time a change, in all respects similar, gives rise to an epidemic rheumatism or catarrh. A third, of the same kind, renders cynanche inflammatoria epidemic, and a fourth produces affections of the eyes. Add to this, that, at other times, vicissitudes, not perceptibly different, generate no diseases at all.

These, I say, are familiar phenomena, and must have a cause. Nor can that cause be found in the sensible qualities of the atmosphere. When those qualities are alike, the phenomena are widely different from each other; and the reverse. In the insensible qualities, then, which make up its constitution, must the cause be looked for. Nor will the search be in vain. The constitution of the atmosphere is the remote cause, forming the *predisposition*, and its sensible qualities, especially its vicissitudes, the exciting or immediately productive cause of the complaint. One constitution predisposes to peripneumony, another to catarrh, a third to rheumatism, and a fourth to ophthalmia, while the same change from heat to cold, or from dryness to humidity, is the common exciting cause of the whole. If there exist no general predisposition to disease, the change, except to valetudinarians, will be harmless. So true is it, that, were it not for the existence and operation of peculiar atmospherical constitutions, neither endemics nor epidemics could ever prevail. Such appears to be the true explanation of the phenomenon in question. Viewed in any other point of light it seems a paradox.

Shall I be told that, by this mode of solution, I push the doctrine of atmospherical constitutions to an unauthorized extent, and, instead of a reality, present to the imagination a philosophical phantom? I cannot admit the correctness of the charge. For a peculiar effect I only look for a peculiar cause; and the whole economy of nature not only justifies me in this course, but counsels me to pursue it. A fundamental principle, without which reasoning would be conjecture, and inductive philosophy but a name, is, that, under similar circumstances, similar causes produce similar effects. Of course, the converse of this must be equally true. To produce dissimilar effects, similar

causes must be under dissimilar circumstances. But it has been shown, that similar states and mutations of the weather give rise to dissimilar complaints. The inference, therefore, is plain. The circumstances under whose influence the mutations operate must be different. But it is alone in a secret constitution of the atmosphere, made up of its insensible qualities, that those circumstances can consist. Imagination can refer them to no other source.

Analogy is favourable to this mode of reasoning: and we sanction it by our daily practices. Different febrile diseases, of very strong and striking characters, we never hesitate to derive from different constitutions of the atmosphere. So urgent is our propensity to this effect, that we may safely call it an instinct. To no other source do we think, for a moment, of ascribing pestilence, yellow fever, influenza, or epidemic measles. And wherefore do we have recourse to this mode of accounting for them? The answer is obvious. We are forbidden, by every consideration that bears on the subject, to ascribe them to the sensible qualities of the air. No tangible cause of any kind presents itself. Hence, by a principle of reason, which we cannot resist, we refer them to a cause that is itself hidden from us, and whose effects alone we are permitted to see. We thus reason, I say, from a feeling of instinct, which we can no more extinguish, than we can eradicate our propensities to eat, drink, love, or indulge in any other enjoyment. Nor is the impulse of the latter more irresistible than that of the former. The process of reasoning is as much the result of a necessity of our nature, as any other that belongs to us. Besides, it is the only means by which we pursue our march from the known to the unknown; that we "look through nature up to nature's God." Extinguish in us this principle, and our gaze on nature will be vacant and unmeaning. We shall wonder, but not reason; admire, perhaps, but not philosophize. We shall neither "see God in clouds, nor hear him in the wind." An acquaintance with mere objects of sense will constitute the entire amount of our knowledge.

I have alleged that we ascribe to corresponding constitutions

of the atmosphere, as their native sources, yellow fever, plague, and influenza, because they are striking and impressive complaints, and therefore awaken much attention. But are epidemic rheumatism, pleurisy, and ophthalmia less real, merely because they are less violent and more common, and excite comparatively but little attention? Or are they, as effects, less essentially connected with appropriate causes? No one will reply to these questions affirmatively. In etiology great effects have no superiority over small ones, except merely *as* effects. Of whatever magnitude they may be, they are all the product alike of specific causes; great effects, of powerful causes; smaller ones of those that are weaker. The principle being thus established, the issue contended for is conceded. If it is admitted that powerful epidemics are rightfully attributed to hidden constitutions of the atmosphere, because its sensible qualities are inadequate to their production, a similar concession will not be withheld, as relates to other epidemics, for no other reason but because they are weaker. As respects their origin, justice requires that the strong and the feeble be treated alike. It is thus that nature treats them. We have the sanction of reason, then, in ascribing to some infectious miasm, in the atmosphere, every fever that assumes an endemic or an epidemic form. Nor do I know of any other source to which they can be referred.

I am indeed aware, that certain epidemics are attributed, by medical writers, to what they call an "influence" or "a state" of the atmosphere, independently of the presence of a material poison. But on me such an effort at explanation is lost, because it is unintelligible to me. In general philosophy, I know nothing of properties, except as they belong to substance; nor, in medical science, can I recognize any "influence," except as the result of something material. If, in its "state" and "influence," the atmosphere is different, at one time, from what it is, at another, the cause is to be found in material agency. Is it warmer?—it has more caloric. Is it less transparent?—it contains more smoke, exhalation, or dust. More humid?—it is loaded with a greater amount of moisture. Does it produce remittents, yellow fever, or oriental pestilence?—it is adulterated

by some kind of febrile miasm. Does it give rise to influenza, scarlatina, or hooping-cough?—it is contaminated by miasm of another description. Is it productive of epidemic pleurisy, rheumatism, or ophthalmia?—the effect must be attributed to a similar cause; the presence and operation of a febrile miasm. No reason can be assigned, why such a poison may not throw inflammation on the pleura, muscles, joints, or eyes, as readily as on the stomach, intestines, or skin.

Shall I be told that all this is nothing but speculation, and needs proof, before it can be recognized as genuine science? I acknowledge that it is not mathematical demonstration; nor is it offered as truth, that must not be questioned. But I contend that it rests on the same basis with other opinions, which are received as true. A belief in the existence of atmospheric miasms and their influence in producing all endemic and epidemic fevers, stands on precisely the same footing. The opinion ascribing intermitting fever to such a poison, is no better supported than that which refers to a similar source, epidemic pleurisy, ophthalmia, or rheumatism. In each case the march of inquiry is from the known to the unknown; from visible effects to an invisible cause. And the induction is as fair in one instance, as in the other.

I am prepared to apprehend, that my opinion respecting the formation of atmospherical constitutions will be deemed heterodox. It will not, I suspect, be immediately conceded, that, for the production of every different constitution, a different infectious miasm is requisite. And yet I perceive no other view of the subject, which is either rational, or intelligible. No other indeed occurs to me which is even plausible. Influenza, scarlatina, measles, and peripneumonia typhoides are all specifically different from each other, and, as epidemical diseases, have their origin in the atmosphere. From the atmosphere therefore, *in different conditions*, they must be derived. That they cannot spring from the *same* condition of it, appears from the fact, that they never co-exist in the same place. But that those different conditions can arise from any other cause than the presence of different miasms, would seem impossible.

I know not how it can be reasonably doubted, that, for the production of influenza, it is requisite that one kind of febrile miasm should take possession of the atmosphere, for the production of measles, another, for that of peripneumonia typhoides, a third, and, for that of epidemic pleurisy and rheumatism, a fourth and a fifth. And the same appears to be essential to the production of every other complaint that can assume an endemic or an epidemic form.

Will it be alleged, in opposition to the views I have submitted, that the different effects alluded to, may arise from different proportions and combinations of the common elements of the atmosphere? I answer, that facts do not countenance such an allegation. Experiments show, that, as far as its "*common* elements" are concerned, there is no difference between an epidemic atmosphere, and a healthy one. Besides, admit the hypothesis, and my views are not impugned by it. The *unusual* combination thus formed, will be the epidemical miasm, for whose existence I am contending.

I am aware that some pathologists attribute the different endemics and epidemics to differences, at the times of their prevalence, in the constitutional predispositions of those whom they attack. The inhabitants of a community or place, say they, sustain attacks of intermitting, yellow, typhus, or scarlet fever, according as they are predisposed to the one or the other. And this they call an explanation. But they misname it. It explains nothing. It announces a truism, known to every one, and goes no further. That the different predispositions referred to exist, cannot be doubted. Without them the diseases could not have being; for it is an axiom in pathology, that all attacks of different epidemics and endemics, are necessarily preceded by corresponding predispositions. But it is those predispositions, in an especial manner, that testify to the existence and influence of the atmospherical constitutions for which I am contending. Of that influence they are themselves the immediate effects. The constitutions do not, by their own direct operation, produce disease. They only predispose to it. For its actual production, an exciting cause is necessary. And this cause is often found

in the vicissitudes of the atmosphere. Hence, as already stated, similar vicissitudes prove productive of dissimilar complaints, in consequence of the existence of dissimilar predispositions. Further exciting causes are, improprieties in diet and drink, excessive indulgence in other animal gratifications, exhausting fatigue, severe exposure, inordinate watching, and paroxysms of grief, fear, resentment, and other passions.

A predisposition to disease, induced by a febrile miasm, is like a predisposition inherited from unsound ancestors. It is not the disease that is inherited. The descendant is not always positively sick. His inheritance is only a predisposition or tendency to sickness, for the real production of which the action of an exciting cause is necessary. In like manner, an atmospherical constitution does not, I say, generate actual disease. It only begets a predisposion, which is converted into disease, by the requisite excitement.

There is yet another point, respecting which my opinion will be deemed by many unfounded. It is, whether measles, scarlatina, and hooping-cough are contagious complaints? The contagion of the two latter I have positively denied, and have done virtually the same, as relates to the former. The following are some of the facts, by which I consider my opinion sustained.

1. In commencing their epidemic career, those diseases uniformly attack individuals, who have not been exposed to sources of contagion. They begin thus in interior situations, where they have not been previously for many years; and in the United States, they often, in this way, make their appearance in new settlements, where they have never been before.

2. They do not attack a single individual, nor even a single family, and spread from thence, in regular and gradual progression, from the sick to the well. They attack at once many individuals or even the families of a whole village, town, or neighbourhood; and their spread is rapid and irregular; much more so than that of diseases propagated by contagion. Nurses and attendants are not more frequently or certainly attacked, than those who shun communion with the affected. They pass over families, and even whole neighbourhoods, to attack others,

at a distance; and, returning on their path, visit those whom they had previously spared. Nor are persons having casual intercourse with the sick more liable to the complaints, than others who have had none.

3. When those diseases are prevailing epidemically, if a case of either of them be conveyed without the epidemic atmosphere, the complaint never spreads, but terminates with the recovery or death of the person thus removed. This is as true of measles as of any other febrile affection. Provided the atmosphere be free from epidemic adulteration, the complaint is no more communicable than gout or rheumatism. Such at least, is the result of the reiterated observations of the writer of this paper.

4. The diseases under consideration cannot be propagated by inoculation; nor can the contagionists designate the tissue by which the miasm is secreted. Hence the well known discrepancy of their opinions on the subject. Some point to the lungs, as the secreting organ, asserting that contagion is conveyed by the breath. Others declare the liver to be the source of mischief, and the bile, of course, to be impregnated with the miasm. A third class pronounce contagion to be secreted by the stomach, a fourth by the intestines, especially the rectum, a fifth by the skin, and a sixth by the fauces and salivary glands. And, that he might certainly include the part offending, the late Dr. Good contended, that contagion is produced by every secreting organ of the body. This warfare of opinion speaks but one language; and that may be easily and briefly interpreted. The disputants are ignorant of the subject of controversy. Such indeed is the true interpretation, as respects every disputed case, where a number of individuals equal in talents and attainments, and who have paid the same degree of attention to the point at issue, maintain each a different opinion.

5. Those diseases cannot, as already intimated, co-exist in the same place; nor, while either of them prevails, does it permit, within its sphere, the existence of any other febrile complaint. Or, if it does not extinguish other forms of fever, it assimilates them to itself. In the language of a popular writer, it "compels

them to wear its livery," and "do homage to it as the sovereign of the day."

Of these facts the explanation is obvious. The complaints in question are epidemic. They have exclusive possession of the atmosphere, by means of their miasms. No predispositions except to themselves can be created, and, of course, no febrile diseases but themselves, or such as are closely assimilated to them, can occur.

But contagious diseases, *merely as such*, never take possession of the whole atmosphere. They are confined to the *personal* atmospheres of those affected by them; or, at furthest, to the atmospheres of sick rooms and infirmaries. Hence they prevail in the same place, and at the same time, with other febrile affections. It is only when a peculiar atmospheric constitution is formed, by some kind of infectious miasm, that but one description of fever can prevail.

6. They begin and end somewhat abruptly, are rapid in their march, as already mentioned, and their duration, which is in some degree definite, is rarely protracted. Of contagious affections the reverse of this is true. Their beginning and termination are much more gradual, their progress slower, and their duration, always indefinite, is, at times, very long. It may be added, that in most, if not in all truly contagious diseases, the contagion-secreting tissues can be distinctly designated. But in the complaints which I am considering, no such tissues can be shown.

Such are a few of the reasons of my disbelief in the contagious nature of these diseases. Further arguments to the same effect might be adduced. But they are deemed unnecessary. If the foregoing are fallacious, and can be refuted, others no stronger would not be likely to have a better effect.

I know it is very generally contended, that measles, scarlatina, hooping-cough, and a few other complaints, are *necessarily* contagious, because the human system is liable to be attacked by them but once. The hypothesis cannot be maintained on this ground. The reason rendered in favour of it has no weight. Were I to inquire of its advocates, what necessary

connexion there is between a secreted poison and a liability to be but once attacked by it? their only true and rational answer must be, "we do not know." Fact, then, does not sustain them. Nor does even analogy. Sundry secreted poisons are known to attack the human system more than once. There is, however, in almost every poison, whether secreted or not, a tendency to weaken, if not extinguish susceptibility, as relates to itself. No poison attacks the same person so readily or severely a second or third time, as a first. This is the only ground on which the process of acclimation can take place. Without it, the requisite change in the human constitution could not be produced. Hence the liability to disease would remain. The hypothesis, then, against which I am contending, is an *error*, which, resulting from a superficial examination of things, time has sanctioned, but which observation and reason unite to overthrow.

Of the origin of the several miasms which have been noticed I have not spoken. Nor need the ground of my silence on the subject be concealed. I can say nothing respecting it satisfactory to myself, or which I could expect to be deemed satisfactory by others. I am indeed ignorant of their origin. From what source, or by what means, the atmosphere becomes impregnated, at different times, with the miasms of influenza, measles, scarlatina, hooping-cough, peripneumonia typhoides, and epidemic pleurisy, rheumatism, and ophthalmia, physicians may conjecture, but cannot discover, in the present state of medical science. And as I have no wish to add to the number of existing hypotheses, I shall not hazard an opinion on the subject.

But there remains a miasm more interesting and important to us than all the others, at whose existence and action I have hinted. Of this our knowledge is supposed, at least, to be somewhat more extensive and accurate. I allude to the miasm of marshes, to whose influence is attributed every modification of autumnal fever. Nor is its action limited to the complaints of autumn. Throughout the year it is busy, in some climates; and, even in our own, we feel its effects in spring and summer. Nor are we free, in winter, from its lingering action on those who had suffered from its influence during the preceding au-

tumn. Thus does it run perpetually its circle of mischief. I proceed now to the consideration of this poison, and purpose to speak of it somewhat in detail.

Of all febrile miasms, that of marshes is the most ancient, universal, and destructive. In these respects it has no rival. If, once in a term of several years, the miasms of small-pox, measles, influenza, and scarlatina, overrun tracts of country of considerable extent, that of marshes exhibits its effects in every populated country of the globe, during a portion at least of every year. All other miasms appear, from their history, to be comparatively of modern date. The ancients had no knowledge of them. Or if they had, they have failed to leave a record of it. But the existence of marsh miasm is coeval with that of the human race. If the views entertained of its origin be correct, its commencement must have been contemporary with the first decay and dissolution of animals and vegetables. According to the present system of physical influences, its production seems to be as necessary, and as much in harmony with the laws of nature, as the descent of ponderous bodies, or the growth of plants. Since the first establishment, then, of the present order of things, it must have existed, and produced its effects.

In our most ancient writings, those effects, if not expressly recorded, are virtually alluded to. The plagues of Egypt were as much the offspring of the miasm of the Nile, in the days of Pharaoh and Busiris, as they are at present. Since the subsidence of the first inundation of that river, its banks must have been a hot-bed of this febrile poison. Wherever, in the habitations of Israel, or among the nations around them, the pestilence "walked in darkness" the virus was present.

The pestilence which desolated the Grecian camp, on the plain of Troy, is ascribed by the poet to the resentment of Apollo. But the philosopher derives it from the miasmatic exhalations of the Simois and the Scamander.

The description of the plague of Athens, by Thucydides, is a vivid and memorable record of the same miasm. So are nearly all the writings of Hippocrates. For almost every case

of disease he has described, appears to have been the product of that poison. So is every pestilence that has depopulated cities and countries, whether in Europe, Asia, or Africa, in modern times, as well as every occurrence of yellow fever, on the continent of America, and in the adjacent islands. All these, I say, are the offspring of the miasm of marshes.

Nor do they constitute the entire amount of the mischief it produces. It is the source of all the intermittents, remittents, common bilious fevers, choleras, and most of the diarrhœas and dysenteries, that destroy such myriads of the human race. It produces also bilious colic, jaundice, hepatitis, and other affections of the liver, inflammation and enlargement of the spleen, dropsy, elephantiasis, and several other chronic complaints. Some of the latter of these are the sequelæ, rather than the immediate effects of its action.

Such are the physical evils produced by this ancient, powerful, and universal poison. When to these are added its moral effects, the distress of relatives and friends on account of the sufferings and uncertain fate of the sick, the affliction of the living for the loss of the dead, the desertion of cities during the prevalence and desolation of plague and yellow fever in them, the restraints imposed on commerce by quarantine establishments, and the injury to business, and loss of property, with the consequent ruin and want, which such events and measures produce—when the entire result is thus summed up, the aggregate of the evil and misery is appalling. But, to the influence, direct and indirect, of the miasm of marshes must the whole be ascribed. A correct knowledge of that poison, then, in its origin, relations, and laws, but more especially a knowledge of its nature, could that be attained, is important to some of the weightiest regulations of civil society, and to the health and welfare of the human race. A desire to aid, however feebly, in the promotion of such knowledge, is my leading object in preparing this memoir.

Notwithstanding the immemorial existence and ravages of marsh miasm, it is not yet a century and a half, since the attention of physicians was first particularly directed to it, as an ob-

ject of inquiry. Previously to that time its very being does not appear to have been thought of, and of course, it had not yet received a name. Its devastations were gazed on with feelings of sacred horror and superstitious awe, as if they had been the visitations of an offended Deity. And to that source, or the physical influence of the heavenly bodies, were they generally attributed. But as the shadows of the dark ages passed away, and observation and rational induction took the place of mere abstraction and hypothesis, the light of truth began to shine on medicine, as well as on other branches of science. Directed by this, Lancisi, an Italian physician of great distinction, was the first to suspect the existence of marsh miasm, to make it a subject of serious investigation, and to bestow on it the name which it still retains. He was born in the year 1654, filled, for thirteen years, the chair of anatomy, in the college of Sapientia, was then appointed ARCHIATER to Pope Clement XI, and resided afterwards in Rome.

The summer and autumnal diseases of the Campagna di Roma, and the Pontine marshes, once the seat of health, and the country residence of much of the wealth and fashion of the imperial city, had been long the terror and scourge of that part of Italy. Lancisi, with a peculiar aptitude for original observation, but delighting chiefly in the study of etiology, could not remain content with mere efforts to cure those formidable complaints. That was to act only the part of a common practitioner. But his aim was higher. He aspired to the achievements and rank of a philosopher and discoverer. Nor was he disappointed in his ambition. After a profound investigation of the subject, he ascertained, to his satisfaction, that the diseases were the offspring of an aerial poison, produced by the dissolution of dead organic substances. And, as that process was most active and abundant in marshy places, he denominated the virus thus generated "*paludum effluvium*," marsh exhalation. In his "*Dissertatio de noxiis paludum effluviis*," a work rich in matter, and otherwise of great excellence, he promulgated his discovery, and expounded his doctrine, with a full detail of the facts that support it. This was about the close of the seventeenth

century, a period not much distinguished by the true spirit of philosophy.

For a time Lancisi shared the usual fate of original discoverers and independent thinkers. He was opposed and traduced by envious contemporaries and selfish competitors, ridiculed by punsters and affected witlings, and denounced by fanatics. By the first class he was called an "impostor" and a "speculator," and by the second a "misty" and "vaporous" philosopher, while the third declared him an "enemy to the faith," because he dared to attribute to an earthly agent, complaints which they derived from Heaven, as a righteous judgment on the ungodliness of the land. But by the clearness and force of his writings, aided by the weight of his character, he triumphed over opposition, and his views of marsh miasm, received as authentic, became the settled doctrine of the medical world. If an opponent of it occasionally appeared, his influence was limited, and his hypothesis never outlived himself.

True, even at the present day, there are some who disbelieve in the existence of the poison, attributing autumnal and other fevers to the action of the sensible qualities of the atmosphere; especially to vicissitudes in its temperature and moisture. But their opinon is untenable. A solitary but well known fact completely subverts it.

When the yellow or any other form of bilious fever prevails in a seaport town, or on the coast of navigable water, mariners, who go on shore, are peculiarly liable to it. But, provided they never visit the shore, but remain constantly on board of a ship lying at anchor, only a cable's length from land, they retain their health. This fact is notorious, and easily explained. The febrile miasm from the shore does not reach them. To any difference in the sensible qualities of the atmosphere the phenomenon cannot be attributed. No appreciable difference exists. Those qualities are precisely the same on shore, and six or seven hundred feet distant from it. The presence of the poison in the former situation, and its absence from the latter, constitute, between them, the only difference, to which, in expounding the phenomenon, reason can attach the slightest weight.

Again; yellow fever is not unfrequently arrested in its progress, by the intervention of a single street. The inhabitants on one side of the street suffer from it, while those on the other escape. But the sensible qualities of the atmosphere are on both sides alike. The only difference consists in the presence of febrile miasm on one side, and its absence from the other.

Besides, how, by the sensible qualities of the atmosphere, can dissenters explain the existence of influenza, scarlatina, peripneumonia typhoides, and other epidemics? To something else than mere heat, moisture, and vicissitudes in the atmosphere these diseases must be attributed. They cannot spring from the same causes with intermittents and remittents, any more than apples and oranges can be the native productions of the same tree. They must be acknowledged to be the offspring of deleterious miasms. But if, as relates to them, this concession is necessary and admissible, it is not less so, as respects the various forms of bilious fever.

Further. In any tract of country, whose climate is variable, an extensive marsh or morass exists. A summer drought prevails, and its waters are much diminished by evaporation, leaving a large portion of its bottom nearly dry. The consequence is, the generation of marsh miasm, and the prevalence of fever. During the succeeding summer the temperature of the weather is equally high, and its vicissitudes equally numerous and great; but rain falls in abundance, and the morass is flooded. The issue is, the existence of general health. To what is the difference in the phenomena to be attributed? Not to any difference in temperature and vicissitudes. In both seasons they are alike. And, in the latter, moisture, to whose influence some pathologists ascribe fever, is much more abundant than during the former. To the presence of marsh miasm alone is disease to be attributed, during the first season, and to its absence, the prevalence of health during the last. Add to this, that in no place does autumnal fever appear, as an endemic, in which the source of its poison may not, by careful inspection, be discovered. Those, therefore, who deny the presence and influence of this miasm, in the production of fever, attribute the disease to causes

that are inadequate. Mere moisture, and vicissitudes in the temperature of the atmosphere, cannot produce it. Were this the case, no small island could be free from it; nor could a ship's crew, in tropical climates, or during the summer, in any climate, ever navigate the ocean in health. A marine atmosphere is necessarily humid; and the vicissitudes in its temperature are frequent, and sometimes great. Yet, in a clean ship, mariners are always healthy at sea; and some small islands, whose atmosphere is entirely marine, are among the most salubrious spots on earth. Autumnal fever, then, is certainly the product of an aerial poison.

But although the existence of this miasm is considered certain, nothing is known of its peculiar nature. On that point all is darkness, or, at best, conjecture. On the atmospheres of marshes, cities, and other places, where autumnal fevers prevailed epidemically, many experiments have been carefully made. But they were made in vain. No lurking poison was detected. By the test of the best eudiometers, used by the most skilful and dexterous experimenters, no difference in purity is discovered between the air of the ocean and the land, the mountain and the valley, the city and the country, the healthy champaign and the sickly morass.

But it is not to be hence inferred, that no difference exists. The contrary is unquestionably true. A difference does exist, as results demonstrate; but it has not been ascertained wherein it consists. The reason is obvious. The means employed are unsuitable. They are calculated to ascertain what proportion of oxygen the atmosphere contains, but nothing more. No test for an aerial poison has been yet discovered. Hence, even in the atmosphere of the wards of a small-pox infirmary, which persons unprotected by vaccination or otherwise, could not enter without the hazard of an attack amounting almost to certainty, no variolous miasm can be detected. There is reason to believe that metallic substances can be converted into vapour, and thrown into the atmosphere, in such a condition, as to escape detection by any experiment that can be instituted for the pur-

pose. So little do we know of the real composition of the air that we breathe.

But in lieu of *discovery*, as relates to the nature of marsh miasm, *conjecture* has been prolific. The imagination of Ovid never teemed with such a brood of metamorphoses. Nor was the offspring more incongruous and monstrous. One hypothesis identifies the poison of autumnal fever with nitrogen, another with oxygen, a third with carbonic acid gas, a fourth with carbonic oxid, a fifth with nitrous oxid, a sixth with hydrogen, a seventh with carburated hydrogen, an eighth with sulphurated or phosphorated hydrogen, and others with other aerial substances, until the resources of hypothesis have been fairly exhausted. So vague and limited, I say, is our knowledge of what the atmosphere really contains, notwithstanding the confident tone in which chemists speak and write on the subject.

In as much, then, as we are ignorant of the *nature* of marsh miasm, what is the amount of our knowledge in relation to it? In particular, what answer can be rendered to the following "Boylston medical prize-question," proposed by the corporation of Harvard college, for the year 1830?

"*Whether Fever is produced by the decomposition of animal or vegetable substances; and if by both, their comparative influence?*"

To reply to this question fully and conclusively, may be pronounced impossible. The present condition of etiology does not admit of a result so definite. In a matter of such interest to science and humanity, our knowledge of the difference between the products of the decomposition of animal and vegetable substances, is too limited to be confidently relied on. There is, indeed, reason to believe, that, in many cases, the difference, if any exist, is exceedingly slight; so slight, as to be, for any practical purpose, wholly inappreciable.

Of a large proportion of the lower orders of animals, especially of such as inhabit the water, and burrow in the ground, the component parts are almost identical with those of vegetables. With those of some vegetables their identity may be pronounced complete; at least so far as analysis has been carried. The difference, in this respect, between vegetables and the

entire verminous and insect tribes, is very inconsiderable. It is not credible, therefore, that the difference can be great between their products, in any chemical changes they may sustain. But it is chiefly the lower orders of animals here referred to, that die, and are decomposed, in great abundance, during the summer and autumn. It is but rarely that those of higher standing perish and putrefy, in sufficient masses to do much mischief. Except on the field of battle, in besieged towns where famine and pestilence already prevail, and in ancient cemeteries, such an event is perhaps unknown. Although injury may be done by the dissolution of smaller quantities of such matter, its sphere is limited. It is with a reference to the death and decomposition of the lower orders of animals, then, much more than of the higher, that a reply must be prepared to the Boylston question.

It will be perceived that, in making these remarks, I consider the question as relating exclusively to the fevers of summer and autumn, denominated bilious; and not to typhus fever. Of the causes of this latter complaint my views are different. When it occurs in jails, hospitals, ships, and other crowded, foul, and unventilated places, its origin appears to be purely animal. And the matter producing it is that which is exhaled from the human body, converted into a poison by chemical agency. It must consist chiefly of the perspirable matter, that which issues in vapour from the lungs, and possibly of the exhalations from alvine discharges. When suffered to accumulate in a confined and stagnant atmosphere, the temperature of which is sufficiently high, these matters pass into a poison of great virulence. But it is not contagious. It cannot I mean, by the morbid action it excites, generate its own likeness. When first secreted it is innocent, but it becomes deleterious by a chemical process, which it undergoes, after its elimination from the system. But this process does not appear to be genuine putrefaction. It would rather seem to be a mode of change *sui generis*, of whose nature we have no knowledge. Nor do we know any thing of the poison it produces, except from its ef-

fects. Toward the close of this memoir I shall make it the subject of a few further remarks.

In reply to the question proposed by Harvard, I have no hesitation in expressing my belief, that marsh miasm, the remote cause of bilious fever, is the product chiefly of vegetable matter, in the process of *decomposition*, but not, I apprehend, of real *putrefaction*. The poison exists where no signs of putrefaction appear. Hence by an indiscriminate and indefinite use of that term, obscurity has been thrown on the subject, and obstacles to the ascertainment of truth created.

I have alleged that the miasm of bilious fever is produced *chiefly* by the decomposition of vegetable matter. But perhaps it is not thus exclusively produced. The decomposition of the lower orders of animals, that perish during the summer and autumn, acts probably as an auxiliary source. It cannot, however, I think, be a very fruitful one, on account of the comparatively small quantity of matter concerned in it. It cannot be doubted that the vegetable matter annually decomposed amounts to many thousand times its bulk. And as its composition is nearly the same, the quantity of miasm it produces must bear to that produced by the animal matter a similar proportion.

When, by excessive falls of rain, the erection of mill-dams, the obstruction of water-courses for the purposes of manufactures or navigation, or by any other cause, considerable tracts of land are flooded, the consequence is the destruction and decay of large quantities of vegetable matter. We know this to be true, because we see the vegetables passing into dissolution in great abundance. But, as respects the death and dissolution of animals, the case is different. Of them but few are ever seen by us. Nor are we authorized to fancy them where we do not find them.

But under the circumstances here represented, miasm is generated, and fevers prevail. Nor, in the places designated, does the poison appear, except under the circumstances just specified. As respects its origin, therefore, the inference seems plain. It is the product chiefly of vegetable matter undergoing decomposition. After the vegetables originally flood-

ed and destroyed have passed into entire decay, and been dissipated in gas, or otherwise carried off, the fever of the place ceases. Hence old mill-ponds are much less deleterious to those who live adjacent to them than new ones. Yet probably the number of worms, insects, and other animals, that annually die about their borders, and suffer decomposition, is as great as previously.

Around the margin of marshes, and in fenny districts, where, in some form, bilious affections annually prevail, decaying vegetable matter abounds. But observation teaches us that the amount of animal substance, in the same condition, is very inconsiderable. The same is true, in every district of country, where, during summer and autumn, bilious endemics prevail. In all such places, then, it is evident, that the febrile miasm is the product of vegetable much more than of animal substances. But of the exact proportion which the two kinds of matter bear to each other, no one is privileged to speak; because, I believe, no one has instituted experiments for the solution of the problem. Nor do I perceive in what way experiments to that effect can be instituted, with any prospect of success. In South Carolina the cultivation of the indigo plant formerly, and of the rice plant, at present, has been found to be productive of bilious fever. Here again, as far as observation may avail, in ascertaining facts, it is vegetable matter alone that undergoes decomposition, and generates the poison. But I know of no instance on record, nor have I ever witnessed one, where masses of putrefying animal substances, have produced, under similar circumstances, intermitting or remitting fever. If they have been suspected of giving rise to a few individual cases of disease, they have not been the cause of an endemic or epidemic. It may be received, then, as a principle, that, in the open atmosphere of the country, autumnal fever is the product chiefly of vegetable decomposition.

But how stands the case in large cities, where common bilious fever rises to a higher grade, and yellow fever occasionally prevails? Is the miasm here also the product chiefly of vegetable matter? To answer this question with definiteness is im-

possible. The filth of a large city consists of such a mixture of animal and vegetable feculence, that the one cannot be distinguished from the other; and, therefore, no inspection can discover, in many cases, which predominates. Might I indulge in mere opinion, however, I would say, that, here too, vegetable substances are most abundant.

But if we take, as our guide, the reputed origin of yellow fever, in our large seaport towns, we shall probably find it equally referable to animal and vegetable decomposition. In 1793 the commencement of that disease is believed to have been satisfactorily traced, in the city of Philadelphia, to a cargo of damaged coffee. In 1797, to a cargo of damaged West India fruit. In 1799, to a few cargoes of damaged and highly offensive hides. In 1803 the disease made its appearance, about the same time, in three distinct places, remote from each other. In each place was a large quantity of unsound oysters, to which it was attributed. Their condition rendered them, on account of their fetor, a serious nuisance to the neighbourhoods in which they lay; and nothing similar existed, at the time, in any other part of the city. In 1805 the fever was again traced to an immense pile of putrid oysters, in the District of Southwark. The stench alone, that issued from this mass of putridity, produced, in many persons, on approaching it, immediate sickness, by its intolerable offensiveness. The same was true of the putrid hides, in 1799. It is worthy of remark, that in the neighbourhoods of these nuisances, several dogs and cats sickened and died of bilious affections, previously to their attack on the human race. To those who have been observant of epidemic diseases, this is known to be a common occurrence. Such complaints are often ushered in by sickness and death among domestic animals, and sometimes among those that run wild in the forest. In 1798 the complaint appeared, in Philadelphia, during the same week, in four or five different and distant parts of the city, and was traced, in each, to a mass of putrefying matter, in some instances animal, and in others vegetable. In the summer of 1819, a most malignant fever, which, in Boston, proved destructive to a number of individuals, was clearly tra-

ced to a cargo of damaged corn. No one was attacked, who had not been in the immediate atmosphere of the vessel that contained this article; and of those who were thus exposed, but few escaped. Nor did the fever cease, in the neighbourhood where the vessel lay, until she was removed to a distance from the wharf, scuttled, and sunk.

The writer of this article witnessed once, in the city of Philadelphia, several cases of yellow fever, which seemed fairly referable to a mass of putrid fish. He would here remark, that as far as his knowledge on the subject extends, the matter of aquatic animals, undergoing the process of decomposition, has been more frequently charged with the production of malignant fever, than that of the higher orders of aerial animals. But, as respects its component parts, the former is more nearly allied to vegetable matter, than the latter. Of malignant fever said to be produced by the putrid carcases of men and horses, on the field of battle, he knows nothing, except from information. He, therefore, forbears to speak of it. Of most that he has here narrated, he has been himself a witness. Hence he speaks of it with the more confidence.

He is aware that some pathologists deny not only the production of malignant fever, but the practicability of its being produced, by the decomposition of masses of animal matter. To sustain their opinion, they adduce instances, in which such nuisances did not generate fever. Hence their inference that it cannot do it.

To sound etiologists it is scarcely necessary to observe, that this form of reasoning is not only inconclusive, but exceedingly hazardous. Its character is negative. It virtually contends, that because an event has not occurred under one set of circumstances, it cannot occur under another; that because it has not been produced at one or two particular times, it cannot be produced at all. But it should never be forgotten, that, in different states of the atmosphere, the results of putrefaction are equally different. As well may it be contended that, because a military leader has never lost a battle, he cannot be defeated. It is scarcely necessary to add, that one positive fact overbal-

ances a thousand such negatives. This topic will be recurred to hereafter.

To all the foregoing reputed causes of yellow fever objections are presented. It is said that the nuisances which have been specified always exist in large commercial cities; and it is therefore, asked, "If such are the sources of yellow fever, why does not the complaint prevail every summer, wherever they are found?" The answer is, that the heat is not always sufficiently intense and long continued; nor does there always exist an auxiliary constitution of the atmosphere. Yellow fever requires, for its production, even in sporadic cases, a certain continuance of tropical temperature; and without a congenial constitution of the atmosphere, it can never, in temperate climates, become epidemic. Were it requisite, facts in confirmation of these two positions could be easily adduced. But they must be familiar to every one competently versed in medical knowledge; especially the knowledge of the diseases of warm climates. The answer, therefore, is deemed satisfactory.

A further objection, however, presents itself. If, at the commencement of yellow fever, the nuisance to which it is attributed be removed, the disease is not eradicated. It not only continues to prevail in the place of its first appearance, but spreads to distant points, and does not disappear, until its extinction by a change of season; the actual passage of the summer and autumnal temperature into that of winter.

The fact, as here stated, is true; and the problem it presents is difficult of solution. When an epidemic yellow fever has begun its career, in one of our large commercial cities, nothing but a termination of warm weather can arrest it. Local nuisances may be removed, the inhabitants of the city may fly, man may erect all his artificial barriers, currents of water may be made to flow along the gutters, rains may fall and wash the entire streets, and the winds may blow, and change the atmosphere of the place. But all to no purpose. If the temperature of the atmosphere continues high, the epidemic mocks at resistance, until it expires under a regular change of season.

How does this comport with the belief, that it derives its ori-

gin from a local nuisance? The question, I repeat, is full of difficulty. I have but one answer to give to it, and that does not please me. The local nuisance, whether it be damaged coffee, fruit, oysters, fish, or corn, emits the febrile miasm first, because it is most matured for its production. It contains the greatest amount of the ingredients requisite to form the poison, and those ingredients are in the most suitable condition for the generative process. Hence the miasm goes forth from it, and the disease begins in its immediate neighbourhood. As the season advances, the common impurities of the streets, which also consist of animal or vegetable substances, or of a mixture of both, are brought into a similar state of preparation. In them, therefore, the same process is excited, and the same changes are produced. Hence they send out their poison, and thus aid in continuing and spreading the disease. And as no human means can remove the whole of them, nothing can eradicate the fever, or stay its progress. The impurities of the streets, now converted into actual nuisances, cover an indefinite extent of surface, and, by means of decomposition, emit every where more or less miasm, until the occurrence of cool weather extinguishes the process. Hence, until that period, the disease continues, and then disappears; as the effect always and necessarily ceases with the cause. Some pathologists have alleged, that the miasm, first emitted from the original nuisances, may act as a ferment, to excite in other masses of filth the process necessary for its own production; and that, by its operation, not on the living human body, but on dead matter, it thus contributes to its own continuance. This being only a conjecture, unsupported by fact, I simply allude to it, leaving to time, under the progress of science, to determine its worth.

To the source of yellow fever here referred to, it is once more objected, that, in large cities, those persons who are concerned in slaughter-houses, tanyards, and soap-candle and glue-factories, where there is much putrid matter, are not more subject to the disease than others who are engaged in cleanlier employments.

To this I reply, that I have often visited and examined the places here designated, with a view to the ascertainment of their

actual condition. My express object was to enable myself to determine, on the best evidence the cases afforded me, whether they were real nuisances, dangerous to the health of the community.

On these occasions I was never able to discover facts, which, in my own opinion, justified a complaint against the establishments. The odour which issued from them was, indeed, unpleasant. But it was not sickening. The animal matter accumulated in them was neither fresh nor sweet. But it was equally remote from being actually putrid. It manifested nothing of that far-gone decomposition, which I have witnessed in other masses of dead matter, suspected as the sources of febrile miasm. To suffer it to pass into such a condition, would render the article useless. In the factories, therefore, it is subjected to the processes of art to which it is destined, before the damage is so deep, as to render it dangerous.

That a slaughter-house might be converted into a nuisance injurious to health, is quite possible. But I repeat, that I have never seen one, the condition of which induced me to believe that it was so. I cannot, therefore, consider the objection valid. Some have even pronounced the exhalations from slaughter-houses favourable to health. In this opinion I cannot concur. Neither science nor experience supports it.

But admit that from the establishments, here mentioned, a gas somewhat deleterious does arise, the mischief done by it need not be great. As is the case with regard to other miasms, those who live in the midst of it become, by degrees, so accustomed to it, that it does them no injury. Thus, acclimated inhabitants are healthy, even in a sickly region, while new comers suffer from the miasm of the place.

Here a question, of some interest, not unnaturally suggests itself. Is the miasm which produces yellow fever identical with that which produces intermittents and remittents? I answer, that although formed from the same materials, I think it is not. My reasons for this opinion are as follows.

The two miasms appear and produce their effects, under conditions of the atmosphere very different from each other.

In the climate of the United States, the miasm of yellow fever requires, for its production, a long continuance of very hot and dry weather; a month or more of tropical heat connected with drought. Under no other state of weather does it seem to be formed. But the miasm of intermittents is generated in an atmosphere cooler and more humid. In the former case the generative process is more intense, in the latter more mild. That the products, therefore, should be different, comports with reason as well as observation. The opinion is strongly supported by analogy; a source of evidence, which, in the absence of positive proof, is worthy of regard.

In other instances, different compounds are formed out of the same elements, according to the strength of the generating process. Thus, under active combustion, phosphorus unites with a larger portion of oxygen, and forms phosphoric acid. Under mild combustion, it unites with less, and the phosphorous acid is the product. But the difference between those two acids, in their strength, affinities, combinations, and general effects, is known to be great.

Of sulphuric and sulphurous acids the same is true. They are formed by processes differing in intensity, and they differ from each other in almost all their attributes. Nitrogen and carbon form also different compounds, according to the quantities of oxygen with which they combine. Of other elementary substances the same may be affirmed. Why not then also of the ingredients, whatever they may be, which enter into the composition of marsh miasm? Of facts opposed to this opinion I have no knowledge; and, I repeat, that reason and analogy support it. Even in the preparation of malt-liquor and wine, the result is different, according to the greater or less intensity of the fermentative process.

But I have yet a stronger and plainer reason for disbelieving in the identity of the miasms of yellow and intermitting fevers. They produce on the human body different effects. No two diseases are more dissimilar, than a case of malignant yellow fever, and a common tertian intermittent. Measles and scarlatina, gout and rheumatism, colic and enteritis, resemble each

other much more strongly. Yet what physician will hazard his reputation by pronouncing them identical?

Yellow fever and an intermittent are not convertible into each other. An intermittent may be changed into a remittent, and the latter into the former, because they are but different grades of the same disease. So, for the same reason, may scarlatina inflammatoria be changed into scarlatina maligna, and typhus mitior into typhus gravior; and the reverse. But, I repeat, that an intermittent cannot be converted into yellow fever, nor yellow fever into an intermittent, because they are diseases radically different.

Nor can they ever coexist in the same place. From the sphere within which yellow fever prevails, intermittents are banished, in common with every other febrile affection. But did they, as some contend, arise from the same miasm, differing only in concentration and strength, this would not be the case. The poison, near to its chief source, where its concentration and strength are greatest, would produce real yellow fever; at a given distance from that point, being more diluted and weaker, its offspring would be remittents; and at the circumference of its sphere, intermittents would be its product.

But a phenomenon like this has never been witnessed. Where its miasm is most abundant, yellow fever is certainly most malignant; at a distance from this it is less so; and near the limit of the district it occupies, it is still lighter. But it is every where yellow fever. Its access, type, and leading symptoms, except as to violence, are uniformly the same.

In Philadelphia, the phenomena of the disease have been strongly corroborative of these views. In that city, yellow fever always commences in Water-Street, where, in the nature of things, its miasm must be most concentrated and virulent; and the complaint assumes there its most malignant form. In Second-Street, about two or three hundred paces from Water Street, and on much higher ground, it is less formidable. in Third and Fourth streets lighter still, and in Fifth street, if it reaches it at all, it is a mild disease; more tractable, and much less dangerous than pleurisy, peripneumony, or a common remittent. But, as al-

ready stated, it is, throughout, yellow fever; as dissimilar to an intermittent in its mildest, as in its most destructive form.

As respects marsh miasm, there remain to be considered several points, of a more practical, and therefore more important character, than some of those that have been noticed. And as the plan of question and answer is most consistent with plainness and brevity, it shall be adopted in the discussion. Conformably to this, it may be asked first;

What are the agents requisite to the production of marsh miasm?

They are, in brief, heat, moisture, and dead organic matter, chiefly, it is believed, of a vegetable character, existing together in due quantity and proportions. But what precisely this quantity and these proportions are, has not yet been ascertained. Nor is it known in what manner the ingredients act on each other, or what peculiar changes they sustain. That a decomposition of both the moisture and organic matter takes place, seems certain. Nor is it less so that a new compound is formed. For it is not credible that marsh miasm is a simple substance. But as already intimated, real putrefaction does not appear necessary to its production. It often exists in great abundance and activity, where no signs of putrefaction can be detected.

If either of the three specified agents be wanting, marsh miasm cannot be formed. It cannot be generated either during cold weather, or under a state of perfect aridity. Nor can it be produced where there are no remains of dead organic matter.

From this view, brief as it is, of the origin of marsh miasm, the inference deducible, as to the prevention of it, is plain. Filth consists in a mixture of water with dead animal and vegetable matter. Preserve perfect cleanliness, therefore, and exemption from the poison is certain. Under that condition of things, it can no more spring up, than the orange can flourish in Siberia, or the palm-seed vegetate on a block of adamant. I the United States, it is impossible to guard against heat and moisture. They belong to our climate, and we could not subsist without them. The cleanliness, therefore, to which I allude, consists in the entire removal of dead animal and vegetable matter. Were this effected, marsh miasm would cease to

exist. Perfect cleanliness would preserve our large commercial cities from yellow fever, with as much certainty as perpetual winter. But, from the nature of things, it cannot be attained. Where human beings are closely associated in crowds and masses, filth must exist. The constitution of man forbids it to be otherwise. To some extent, therefore, marsh miasm will be generated, and fever will be the issue. The utmost man can do, and all, therefore, that he is required to do, is to prevent the excessive accumulation of filth, as the result of inattention, indolence, or neglect. When he has done this, he has performed his duty. Further preventions are beyond his control.

It is nearly ten years since Dr. Ferguson, "Inspector of Army Hospitals" in Great Britain and her Colonies, read, in the Royal Society of Edinburgh, a paper "On the Nature and History of marsh poison". The essay was published in the *Transactions* of that Society, Vol. IX, and republished in Vol. VII *of* the "Philadelphia Journal of the medical and physical sciences."

The article has attracted considerable notice, and made, perhaps, some converts to the opinion it maintains. It is doubtless of no ordinary authority, Dr. Ferguson being a physician of distinction, and having derived his knowledge chiefly from observation and experience. Many of the facts it contains are interesting and important, and some of them are announced by their author, as if they were new. They are, moreover, by perhaps a majority of physicians, received as new. But they are so received by mistake. Whatever novelty may be in the inference, which their author has deduced from them, they themselves, although individually new, are not so, in their nature. Facts precisely analogous are recorded abundantly in medical writings, and to physicians of observation and experience, in the United States, they have long been familiar. It has been, at least in the middle and southern states of the union, long and universally known, that, in the low and flat lands of rivers, bays, inlets, and lakes, in the neighbourhood of mill-ponds and marshes, and in alluvial situations generally—it has been long known, I say, that in places of this description, warm and dry summers and autumns are more productive of marsh miasm, and the fevers

which arise from it, than summers and autumns that are wet and cool. And to this only do the facts of Dr. Ferguson testify. Under a rigid analysis their import goes no further.

In August 1794, the British Army encamped on a low and level alluvial plain, in South Holland. The season was hot and dry, and the troops suffered greatly from intermitting and remitting fever. In the summer of 1799, another British army, commanded by the Duke of York, encamped, in Holland, on similar ground. The season was uncommonly wet and cool, and from intermittents and remittents the troops suffered but little. Still, however, those diseases did occur; and dysentery, which is but a modification of bilious fever, appears to have been troublesome. Here let me observe, that the same general state of things, which, in dry weather, produces remittents, throws diseased action more on the bowels, and gives rise to dysentery and cholera, when the weather is wet. This is a common occurrence, and can be easily explained. In the present case, therefore, the troops being attacked by dysentery, testifies to the existence of marsh miasm. It is not probable that wet weather alone would have created that complaint. In the summer of 1810, a third British army encamped, at Walcheren, "on a soil as similar as possible" to that of the preceding positions, "and certainly not more pestiferous." The season was again "hot and dry," and the mortality occasioned among the troops, by the endemic of the place, "was nearly unprecedented in the annals of warfare."

Our author details a number of analogous facts, which fell under his notice in Spain, Portugal, and the West India islands. His object is to show that the miasm of bilious fever is not the product of marshes. He contends that before this poison can appear, the *marshy* nature of the spot where it is generated must have disappeared, and a state of perfect dryness succeeded. "Exactly" says he "in proportion to the previous drought, and consequent dryness of soil, is the quantum of sickness. I have visited it (Salvaterra near Lisbon) upon these occasions (during its sickly season) and found it the most parched spot I ever saw." In another part of his paper, the Doctor thus expresses himself.

"One only condition, then, seems to be indispensable to the production of marsh poison, on *all surfaces capable of absorption*? and that is the *paucity* of water, where it has previously and recently *abounded.* To this there is no exception in climates of high temperature; and from thence we may justly infer, that the poison is produced at a highly advanced stage of the drying process."—Again. "I think it may be fairly presumed that water, for as long as it can preserve the figure of its particles above the surface is innoxious, and that it must first be absorbed into the soil, and disappear to the eye, before it can produce any mischievous effects."

That Dr. Ferguson has faithfully narrated facts, which fell under his notice, we are forbidden by his character and standing to doubt. Besides, as already mentioned, they are analagous to facts, which repeatedly present themselves to us, in our own country. But, that he has carried his opinion, as an inference from them, too far, we are amply justified in believing and asserting. It is not true that *perfect dryness* is essential to the production of marsh miasm. On the contrary, *it is true,* that under *complete aridity,* its production *is impossible.* As well may its generation from gold or silver be contended for.

From a correct history of the appearance and disappearance of marsh miasm, along the shores of the Nile, much information is derived, in relation to the production of that poison. There are two periods, at which Egypt is entirely exempt from it; when the land is *flooded,* and when it is *perfectly dry.* The season of sickness is when the country is in a state of transition from inundation to *complete aridity.* To be more circumstantial on this point.

Egypt is one of the dryest countries on earth. Throughout the year, a shower of rain but rarely visits it. For all its water, as well as its fertility, it is indebted to the annual superflux of the Nile. About the middle of August, that river begins to overflow its banks. In October it attains its greatest height. During November, December, and January, its waters gradually recede within its channel; and in February or March, according to the character of the season, when the earth

is neither flooded nor dry, miasm begins to be generated, and fever to prevail. The country is now luxuriant in vegetation. In every production, whose flavour, fragrance, and colour can delight, it is a paradise. But, no refreshing showers falling, nor a cloud appearing in the heavens, to intercept even a sunbeam, as the sun advances from the south, and his fervours become more intense, all verdure and blossoms die, and the earth is parched as if by a conflagration. By the time of the summer solstice, the aridity is complete. The source of the dews is dried up, and not a particle of moisture is any where found, in all the land of Sesostris and the Pharaohs, except in the river, and in artificial reservoirs, where it is preserved for use. Neither Spain, nor Portugal, nor any other country in Europe, ever experiences so complete a desiccation.

In this condition of things, about the twenty-fourth of June, the fevers of the country suddenly cease. Why? Because the miasm productive of them no longer exists. Why does it not exist? Because there is no moisture to aid in its production. Heat and vegetable relicks still abound; but humidity has vanished. So instantaneous is now the cessation of disease, that the ignorant and superstitious inhabitants, deluded by their priests, attribute it to the interposition of a tutelary Saint. Hence public processions, thanksgivings, and rejoicings mark the occasion. This statement furnishes, as I trust, a satisfactory answer to the question, so often proposed, why the plague, which ceases, in Constantinople, only at the commencement of cool weather, should cease, in Egypt, during the hottest season of the year?

There are three causes which are equally effectual in extinguishing plague, and all other febrile diseases produced by marsh miasm: flooding, aridity, and cold weather. And they operate alike in preventing the generation of the productive poison. The latter is the agent in Constantinople, and the last but one in Grand Cairo.

One of Dr. Ferguson's opinions calls for a stricter analysis, and a more severe examination, than it has yet received. It has been already quoted, and is expressed as follows.

"One only condition, then, seems to be indispensable to the production of marsh poison, *on all surfaces capable of absorption*: and that is the *paucity* of water, where it has previously and recently *abounded.*"

"One only condition," the surface being "capable of absorption," and having been previously wet; and that is simply that it become dry again! No matter of what that absorbing surface is composed, pure silex, calx, alumine, or magnesia, (for all these absorb) without even a particle of animal or vegetable matter in it; only wet it abundantly, and then let it become sufficiently dry—and the drier the better; for as is "the *dryness of soil*, so is the *quantum* of sickness"—let all this be done, and the product will be a flourishing crop of marsh miasm!! Such is the mode of incubation prescribed, and such the *promised* brood! But we have it *only in promise*, and will never have it in fact. As well may we talk of raising wheat without seed, or hatching chickens without eggs. To be serious.

Suppose a plain of pure silicious sand, free from the slightest relick of dead organic matter. This would be "capable of absorption" in a very high degree. It is flooded, for a week or a month, with pure water, equally free from animal and vegetable feculence, and then suffered to pass through all the stages, to the *maximum of "dryness."* Does there exist an enlightened etiologist who will contend or believe, that, at any point of this drying process, marsh miasm will be generated? I think not. From an answer directly affirmative, Dr. Ferguson himself will shrink. He will not hazard his reputation in an attempt to maintain a position so groundless. In every locality, whether it be the waterless channel of a mountain stream, or a stream in a valley or a plain, where he has witnessed the production and ravages of marsh miasm, *alluvion* existed. Such, in the nature of things, must be the case. When swollen by rains or melting snows, mountain streams abound, in the highest degree, in animal and vegetable relicks, washed by their waters from the adjacent heights. And their currents are never so uniformly rapid, as not to be sufficiently checked, in innumerable places, to make alluvial depositions. There is not, in either Spain or Portugal, a sin-

gle brook, creek, or river, of which this is not true. I have examined mountain streams, as impetuous in their currents as any in Europe; but I have never seen one whose banks and bottoms did not contain deposites of alluvion. And I venture to say, that such is also the condition of every "hilly ravine" in Portugal, through which even a rivulet flows, as well as of the channel of the "river Guadiana." Dr. Ferguson never witnessed the production of marsh miasm, where there was nothing but dry rocks and pure earth, free from all organic relicks. Such a paradox has never been witnessed by any one; nor will it be presented, until the laws of nature change.

Indeed I can scarcely conceive of a situation and general combination of things, better calculated for the production of marsh miasm, than those which the Doctor presents to us, even when he seems to wonder that the poison was produced. Let us briefly examine them, and then decide, whether I do not speak correctly.

After the battle of Talavera, fought in "the hottest weather," the British army retreated "into the plains of Estremadura, along the course of the Guadiana river, at a time when the country was so arid and dry, for want of rain, that the Guadiana itself, and all the smaller streams, had in fact *ceased to be streams*, and were no more than lines of detached pools, in the courses that had formerly been rivers; and there they suffered from remittents of such destructive malignity, that the enemy and all Europe believed that the British host was extirpated."

Such is the picture, given by Dr. Ferguson, of the condition of the country, through which his gallant compatriots passed, and such the general circumstances of the retreat. The weather was intensely hot, the troops, of necessity, greatly fatigued, and the river Guadiana, along which they moved, converted, by drought, into "a line of pools." And adjoining these "pools," as well as in other parts of the channel of the river, were, as certainly as nature works by uniform laws, depositions of alluvion. And yet the Doctor seems himself surprised that the army was sick; and imagines that the information to that effect will surprise others. Let him reverse the matter.

and he will be right. Had the army *not* been sickly, the fact would have been surprising. According to the general views entertained on the subject, a better arrangement for the formation of marsh miasm, than that which our author has depicted, can scarcely be imagined. Each pool, in the channel of the Guadiana, resembled a little mill-pond or lake, in a season of drought, partially exhausted of its waters. Its immediate margin, therefore, to say nothing of the bottom of the river between the pools, was as excellent a laboratory for the preparation of miasm, as memory can recall, or imagination picture. And the same was true of the margins of the "stagnant pools," and the intervals between them, in the bottoms of the "hilly ravines" of Portugal. They were so many well constructed laboratories for the formation of marsh miasm. In describing them, therefore, and reporting their products and effects, Dr. Ferguson has given us no new information. The only point of singularity or surprise, in the whole affair, is, that either he, or any other enlightened physician, should have deemed new, that which is a matter of such general notoriety. The Doctor has simply told us, that he witnessed, in Spain and Portugal, the production of marsh miasm, in places where the whole world knows it is always produced; I mean moist alluvion, acted on by heat.

The experiment on a plain of silicious sand, which I have only *supposed*, is made annually in South Carolina and Georgia, as well as in some parts of Louisiana. In those states, the pine-lands, which are plains of sand, afford healthy retreats to the inhabitants of the maritime and alluvial districts, during the sickly season. Yet, on these plains water falls profusely, and is carried off again, by "drying." But no febrile miasm is produced in the process. Yet here, the sand contains some small portion of vegetable relicks; but not enough to do mischief, under ordinary circumstances. Were it perfectly pure, the security afforded by a retreat to it would be greater.

To say the least of it, the standing of Dr. Lind, as a medical writer, is not inferior to that of Dr. Ferguson. But the entire weight of his authority is opposed to the hypothesis, that the extreme of aridity is requisite to the production of marsh

miasm. As the result of his own observation, he tells us, that, at Senegal, the year is divided into two seasons, the wet and the dry. During the former, rain falls in a profusion scarcely equalled in any other portion of the globe. Yet that is the season of sickness. During the period of the greatest aridity, the region is healthy. As relates even to Holland, one of the countries in which our author made his observations, the authority of Pringle is to the same effect. He represents a certain amount of moisture as one of the requisites essential to the production of febrile miasm. And he also wrote from observation. Further concurrent testimony exists in abundance; while none, I think, of weight, can be adduced in opposition.

Dr. Ferguson is an excellent observer, but an ordinary philosopher. He therefore, reports facts much better than he expounds their causes. In his efforts at discussion, his ideas and expressions are loose and indefinite, and his reasoning feeble. When he speaks of the "putrefaction of water," he evidently has no correct knowledge of his subject. Water, as such, is not susceptible of putrefaction. Nor is any other mineral substance. Nothing can putrefy that has not possessed life. To be putrefiable, or rather to appear so, water must hold in mixture animal or vegetable substances. And it is then the dead organic matter that putrefies, not the water. Pure water is no more susceptible of putrefaction, than pure air or pure gold. Whatever changes it may undergo by stagnation, are not the result of real putrefaction. Nor will they generate febrile miasm. The offensive bilge-water of a ship, which Dr. Ferguson specifies, as an instance of aqueous putrefaction, is impregnated with much vegetable matter. And it is that which putrefies. Water is essential to the process, but is not itself susceptible of it.

I repeat, then, that, as relates to the generation of marsh miasm, our author has not recorded a fact that is new. He has given a valuable collection, well ascertained, and fairly reported, of facts such as were already known; and that is the extent of his contribution to medicine. And for that the profession is somewhat indebted to him. But his opinion, that the amount of

disease bears necessarily an exact proportion to the prevailing degree of aridity, is an error, which the *members* of the profession are called on to reject, and in every way discountenance. If I am not greatly mistaken, the following representation is true, especially in the middle and southern sections of the United States.

When the summer and autumn are moist, and moderate in temperature, intermittents prevail in the places where they are endemic. When the season is very hot, and copious showers occasionally fall, the fevers, in the same situations, are remittents. And when drought and great and continued heats combine, a more severe and malignant complaint, perhaps yellow fever, is the issue.

The history of disease in the valley of the Mississippi, during the summer and autumn of 1829, is in direct opposition to the opinion of Dr. Ferguson. That season was distinguished by an abundance of rain. It was perhaps the wettest experienced in the West since its first settlement. No near approach to aridity prevailed during any portion of it. Yet it was far from being, as, on the Doctor's hypothesis, it ought to have been, the most healthy. On the contrary, it was marked by a greater amount of bilious fever, than had existed previously for many years.

On the subject of unusually wet and dry seasons, as connected with disease, I have a few remarks to offer, which may be introduced into the present part of my memoir, without being altogether out of place. When such seasons are accompanied by an inordinate prevalence of sickness, it is uniformly attributed to the sensible qualities of the weather. In a wet season, the superabundant rain is blamed, and, in a dry one, the excessive drought.

May not the blame, in each case, be, to a certain extent, unfounded? For the fall of a superabundance of rain, as well as for the occurrence of inordinate drought, there must be a latent cause. No *perceptible* agency is sufficient to account for them. But the cause must be meteorological. It must consist in a peculiar condition of the atmosphere, the common source of en-

demics and epidemics. Does it not, then, comport with reason to believe, that the same unusual conditions of the atmosphere which, at one time, give profuse rains, and, at another, create severe drought, may also contribute to the production of disease? May they not be connected with those atmospherical constitutions, which have been already adverted to, as the source of epidemics? And may not, therefore, excessive rains and unusual sickness, in one season, and excessive drought and unusual sickness, in another, be contemporary effects of a common cause? Although I shall not myself reply to these questions in the affirmative, I would not, without greater hesitation, give a negative reply.

It may be here remarked, that both now, and throughout all time to which history extends, epidemic fevers, especially very destructive ones, have been accompanied by extraordinary phenomena, mostly atmospherical, but, on many occasions, connected also with the animal and vegetable kingdoms. Hence, in Scriptural history, pestilence, famine, the locust, and the palmer worm, are often associated in narrative, as having been contemporary. And unusual celestial appearances are stated as having frequently occurred at the same time. During some of the seasons, in which yellow fever prevailed in Philadelphia, igneous meteors are known to have been uncommonly abundant, and, during others, mosquitoes and grasshoppers, were unusually troublesome in the city and its vicinity. And we are informed, in history, that, during the prevalence of some of the plagues of Jerusalem, the "sky was so streaked, at night, by shooting stars," that the superstitious inhabitants trembled at them, as indicative of the anger of heaven. It is further remembered by the citizens of Philadelphia, that, during several of the seasons, when yellow fever prevailed in that place, there was almost an entire absence of lightning and thunder, while, during others, there was a superabundance of them.

On some occasions, when an epidemic has prevailed among the human race, domestic and other animals have been sickly, and many of the productions of the earth, in the same condition. In these instances the complaints of man have been often as-

cribed to the use of damaged provisions. But whence arose the maladies of the lower orders of creation? Is it not likely that the diseases of the human family, of the inferior animals, and of vegetables, were the offspring of a common cause? An epidemic fever is always and essentially of atmospherical origin. Nor is there any thing unreasonable in the belief, that the same condition of the atmosphere, which injures man, may injure also other kinds of living matter.

In 1793, yellow fever prevailed, in its most destructive form, in the city of Philadelphia. So diseased were the livers of hogs, brought to the market of that city, during the autumn and winter of the same year, that they could not be eaten. The disease of horses called the "yellow water" is a bilious fever, and is often contemporary with the same complaint in the human family. It is, moreover, endemial, in the same situations with bilious fever. The cats always, and the dogs sometimes, died in Philadelphia, of bilious affections, in great numbers, during the seasons in which yellow fever prevailed among the human inhabitants. So true is this, that, if, in June and July, the cats began to sicken and die, the citizens looked on the event with alarm, as a premonition of mischief to themselves. In Italy the *aria cattiva* of the Pontine marshes is highly deleterious to black cattle and sheep. It produces in them a malignant bilious fever. So certain is it, that the same atmospherical constitution which sickens man, sickens also his domestic animals. The author of the Iliad, therefore, was no less of an accurate observer than a great poet, when he said of the plague of Troy,

"On dogs and mules the infection first began,
"And last its vengeful arrows fixed in man."

But to return from this digression, and offer a few further remarks on the origin of marsh miasm. Of every fertile soil, more especially if it be *alluvial*, vegetable relicks always, and animal frequently, constitute a portion. These relicks, being greatly comminuted, are in a fit condition to undergo decomposition and change, and, under the influence of the requisite agents, to produce marsh miasm. But, as already stated, those agents are moisture and heat. Of these, the former is always present

in sufficient quantity, except under such a long and burning drought, as that which visits the Delta of the Nile. Expose, then, fairly, at any time, except when it is flooded, for a sufficient period, a tract of rich, especially alluvial soil, to the action of the summer sun, in the climate of the United States, and marsh miasm will be certainly produced. Under what degree of desiccation it will be most readily and abundantly produced, I cannot tell. But I know the soil must be neither very wet, nor robbed of the last particle of moisture. In the latter state it could no more give birth to marsh miasm, than a well burnt brick. In the former, the poison either would not be produced, or it would be absorbed by the moisture, as soon as produced. For it will appear presently, that its attraction for moisture is strong. The following fact is corroborative of the opinion here delivered.

Philadelphia stands on alluvial ground. What is now, in that city, the beautiful promenade, called Washington square, was once Potter's Field, a place set apart for the burial of strangers, and of the poor, and exhibiting an extensive and unsightly collection of neglected graves. It is about twelve or thirteen years since the improvement in it was made. The surface of the ground was uneven, being along one entire side, but more especially in one angle of the square, considerably depressed. This hollow was filled up and levelled, by earth taken from cellars, and the excavations of new streets, and otherwise collected in all parts of the city. The work was completed between the first of October, and the middle of April, of the following year. There was now presented to the sun an area of several acres, covered with fresh alluvial earth, on which his beams had perhaps never acted before. And that earth contained moisture. The consequence was serious, although not signally fatal. As the season advanced, the increasing heat, acting on this mass of humid alluvion, generated a miasm, which produced many severe and obstinate attacks of fever, in the neighbourhood adjacent to that section of the square, where the earth had been deposited. The inhabitants of the other sides of the square escaped, owing, in part, to distance, but chiefly, perhaps,

to the direction of the prevailing winds. The sickness occurred along the south and southeastern quarters of the square; and it is known that the vernal winds of Philadelphia blow chiefly from the north-east and north-west. Every other portion of the city was exempt from disease.

This fever prevailed during the latter part of April, and throughout the month of May, and then disappeared. Nor was the cause of its disappearance hidden. It was the same that extinguishes the plague, and other forms of bilious fever, in Egypt, about the twenty-fourth of June. I mean aridity. The season was dry, and the solar heat had so completely exhausted the alluvion of its moisture, that no more miasm could be produced. On the extinguishment of its cause, therefore, the fever ceased.

Another event, analogous to this, occurred in Natchez, in the summer and autumn of 1820. The police of the city had entered on the process of levelling, on an extensive scale. In some places streets were excavated to the depth of six or eight feet, or perhaps more; and with the alluvial earth, thence removed, hollows in other places were filled up. Thus was a very extensive area of fresh clay exposed to the burning sun and heated atmosphere of that region. The consequence was terrible. A miasm was generated, which produced one of the most malignant and desolating fevers that has visited the South.

Occurrences similar to these are common in the original settlements of almost every new and fertile country. When the inhabitants first arrive, the place is healthy. But no sooner have the axe and the mattock removed the forest timber and the underbush, and the ploughshare opened to the sun beams the bosom of the humid soil, than a miasm is generated, which produces fever. Hence emigrants from the eastern to the western states of the union, especially if they settle on a tract of land entirely new, rarely escape [illegible] But, as cultivation covers the soil [illegible] vegetable matter [illegible] and at the same time prevents from the sun [illegible]

Although these facts concur in proving, that a marsh is not requisite to the production of the poison of bilious fever, they further prove, that a certain degree of moisture *is*. The prevalence of fever, then, is never directly as the degree af aridity. Dust perfectly dry will not produce it. Nor will pure silicious or calcarious earth, with any modicum of moisture that may be mixed with it. Vegetable or animal matter is as essential to its production, as light is to vision, or sapidity to taste.

How far does marsh miasm travel from the place where it is produced?

The science of medicine does not, at present, contain materials to furnish an answer to this question. Facts respecting the real movements of this poison are wanting, and mere conjecture is substituted in their place. Hence it often comes from an adjacent source, when it is supposed to come from a distant one. It is often generated in the rich, and humid soil of fields, open forests, and pleasure-grounds, when it is believed to be the product of a marsh, or a mill-pond, situated beyond them.

The extent it may travel from its source is regulated somewhat by the character of the surface, over which it has to pass. If the ground be hilly, the sphere of its progress will be more limited, than if it were level. Yet it ascends hills of considerable elevation, and produces disease on their summits. But its power is not so great on the opposite sides.

Instances are recorded, in which this poison is stated to have travelled several miles. I have never witnessed any such; nor am I prepared to believe in their existence. I doubt if it ever travels one mile; nor, in ordinary cases, more than half the distance. I repeat, that the belief in its moving so far from the place where it is formed, arises from intervening and nearer sources of it being overlooked.

When yellow fever prevails in a city, its progress, as already stated, is often arrested by the width of a street. It has been known to attack most of the inhabitants, who remained in their houses, on the east side of certain streets in Philadelphia, while those on the west side were nearly exempt from it. And beyond the limits of the city it never passes. To what must this

be attributed? Within a certain sphere, the febrile poison exists in abundance, as its ravages too plainly show. But a few paces beyond that sphere, there is no evidence that it exists at all. The phenomenon admits of but one explanation. The poison, in passing through an uncontaminated atmosphere, becomes immediately neutralized, or so diluted, as to lose its virulence. There is reason, however, to believe, that the form of miasm productive of intermittents and remittents, travels further from its birth-place, than that which produces yellow fever. But how much further no one can tell. It is useless, therefore, to dwell on the subject.

Is this poison capable of being conveyed through the atmosphere by the wind?—It is. Hence, in places where the summer and autumnal wind blows with steadiness, from one quarter, bilious fever prevails much less on the windward, than on the leeward side of marshes, mill-ponds, and other bodies of stagnant water. In such situations, the inhabitants on the former side are often in the enjoyment of health, while those on the latter are suffering from disease. And if the miasm does sometimes produce fever, on the windward side, it is at a much shorter distance from its source, than the bounds to which it reaches on the opposite side.

A knowledge of this fact is important in the selection of sites for human residence, whether in cities, towns, or single dwellings. Is the site to be chosen near to a copious source of marsh miasm? and is the place swept, during summer and autumn, by a prevailing wind? Let the residence be erected on the windward side. Thus, in Virginia, the Carolinas, and Georgia, where the summer and autumnal wind is from the south and west, the inhabitants, on those sides of a marsh, mill-pond, or river, are often healthy, while those on the east and north sides are subject to the endemic of the place. The same wind which carries the poison from the former, conveys it to the latter. I need scarcely add, that miasm is conducted, by the wind, to a greater distance from its source, than it can travel through a tranquil atmosphere.

Should it become necessary to establish a town, or erect a fortifica-

tion, or a single dwelling, near to a marsh, or any other body of stagnant water, and on the leeward side, how may it be best protected from the influence of the miasm?—By draining the marsh, or other stagnant water, and converting it into a meadow, or otherwise covering its surface with dense vegetation. Or, should this be impracticable or inexpedient, by suffering the forest timber and underbush, if there be any between the buildings and the marsh, to stand; and by planting trees and shrubbery there, if there be none. Whether it acts mechanically, or in some other way, a cordon of trees is one of the best safeguards against marsh miasm. In Persia, and other countries of the east, this truth has been long known, and practically applied. Hence, between the dwellings of the inhabitants and contiguous sources of febrile poison, crowded shrubbery, and dense rows of luxuriant and bushy trees are uniformly found.

At a short distance to the south of Philadelphia, lies a large tract of alluvial land, called the Neck. Originally it was marshy, containing much stagnant water, and its inhabitants were annually visited by intermitting and remitting fever. Between it and the city stood a cordon of trees. In consequence of this protection, the inhabitants of the city were secure from the miasm generated in the neck. When the British army had possession of Philadelphia, during the war of the revolution, the protecting timber was cut down by the soldiers, and consumed as fuel. The effect was serious, though somewhat instructive. For many years afterwards the southern extremity of the city, which lies contiguous to the Neck, suffered greatly from intermitting and remitting fever. Nor was it again secure, until, by cultivation, the neighbouring marshes and ponds were drained, and their sites covered with a mat of vegetation. The tract of land, once so unsightly and sickly in itself, and so deleterious to its neighbourhood, presents nothing now but rich meadows and productive gardens, from which Philadelphia is supplied with an abundance of hay, and the finest vegetables her market affords. Since agriculture and horticulture have thus done their work, the inhabitants of the Neck are as free from fever, as those of the city. In Jamaica, Guiana,

and other portions of tropical America, dwellings are, in like manner, protected from miasm by shrubbery and trees.

Which is the most dangerous, exposure to miasm by day or by night?—Exposure by night is, by far, the most dangerous. This answer rests, for its truth, on well known and important facts.

When yellow fever prevailed in Philadelphia, individuals who spent the day, in the city, in attention to business, or for other purposes, but left it before sunset, and passed the night in the country, usually escaped the disease. But those who remained in the infected atmosphere, both night and day, for the most part suffered. As relates to prophylaxis, this is a point of much importance. The writer of this article has the gratification to believe, that by giving directions to men of business, conformably to the principle involved in it, he has prevented much suffering, and perhaps saved many lives. His uniform reply to those who have consulted him has been, "If you cannot quit the city entirely, until the termination of the epidemic, at least sleep out of it every night."

When a ship is lying at anchor, a short distance from a sickly coast, the sailors that go on shore, by day, for wood and water, generally remain healthy, provided they return to the vessel, and sleep on board of her every night. But if they remain on shore a single night, they usually sicken.

The inhabitants of Rome, and strangers who visit that city, can pass the day with impunity in the Pontine marshes. But if they remain there a single night, they rarely escape the influence of the miasm. An attack by the endemic of the place may be considered almost certain. It may be further stated, as a fact which is notorious, that in the maritime and sickly districts of the southern section of the United States, those inhabitants who most carefully avoid exposure, at night, suffer least from the endemic of the country.

As marsh miasm has an affinity for moisture, it is believed to be concentrated, and rendered more deleterious, in the evening, by the descent of the dew. Hence the danger, in a sickly situation, of being exposed to the fall of that meteor. There are two falls of dew during the night. Of these, the first occurs

soon after sunset, and the other between midnight and sunrise. In case, therefore, of night-exposure being indispensable, it is least noxious between the hours of nine or ten at night, and one or two in the morning. Such appears to be the result of experience.

To what elevation above the surface of the ground does marsh miasm rise?

To this question, which possesses much interest, in a scientific point of view, and no little importance, in a practical one, no definitive answer can be given. That the poison does not ascend to a great height, we certainly know; but to what height precisely, we do not know. Our knowledge, however, on this subject, is sufficient to be useful to us, in our professional intercourse with our fellow citizens. We can found on it certain prophylactic measures, if we cannot derive from it such as are curative.

In a city, where the buildings are three stories high, and upwards, say from thirty-five to forty-five or fifty feet, the miasm does not reach the upper stories in the same state of concentration and strength, which it possesses in the lower. It is doubtful whether it reaches them at all. But it has been shown that exposure to that poison, at night, is much more dangerous than exposure during the day. Hence, when yellow fever prevails in one of the Atlantic cities, it is much best for those, who cannot retire into the country, to spend their evenings and nights in the upper stories of their houses. By this practice, health in Philadelphia has been, in some instances, preserved.

The same is true, as respects Norfolk. When yellow or high bilious fever has prevailed in that city, much less sickness has occurred among those who slept in upper stories, than those who slept in lower ones. In the low lands of the Carolinas, the same truth is familiar to every one. During the prevalence of the endemic of the country, those who sleep in the highest chambers are least subject to it. I know not that the experiment has ever been made in New Orleans. But I feel persuaded that it might be made with good effect.

When the plague appears in Constantinople, Smyrna, or Cairo, the Europeans, who reside in those cities, shut themselves

up in their houses, and hold no direct intercourse with those that are without. Their intended object is to avoid contagion; and they generally escape the disease. But why do they escape? Not because they prevent the approach of those who have been attacked by the plague, or exposed to it; but because they reside and sleep on the highest floors of their dwellings. They are above the reach of the poison. Let them pass their whole time in the lowest parts of their houses, and barricade their doors, and fumigate their letters and provisions as they may, the disease will find its way to them. Another ground of their safety is, that, from the nature of their seclusion, they avoid night exposure, and escape exciting causes. Their habits are regular, and they live temperately. All this contributes to their safety.

One of the most distressing and unmanageable of our summer complaints is Cholera Infantum. It is chiefly a disease of large towns and cities. The best and perhaps the only certain mode of prevention is to send children into the country, before the malady attacks them, and allow them to remain there throughout the summer. But the condition of a large majority of families forbids this precaution. Some other preventive measure, therefore, should be substituted. The writer of this memoir has successfully tried the following, induced by the reasons about to be stated.

Cholera Infantum, like other forms of bilious disease, is the product chiefly of marsh miasm. If children cannot be removed into the country, entirely beyond the sphere of this poison, let them be kept, as much as possible, *above* its reach. As already stated, it does not seem to rise, in its strength and virulence, to the highest parts of city dwellings. Under this belief, the upper rooms of houses have been directed to be converted into the family nurseries. In these the children slept, at night, and were kept in them throughout the day, except when taken out for exercise; and then they were conveyed immediately out of the city, to enjoy, for a short time, the pure air of the country. This experiment, tried in a number of instances, so far succeeded, as to prove satisfactorily the correctness of the princi-

ple, on which it was instituted. During the prevalence of yellow fever in one of our cities, physicians, whose profession does not permit them to fly from the danger of it, may contribute to their own safety, by sleeping in upper stories.

But although marsh miasm does not ascend to a great height in the atmosphere, it climbs to the summits of lofty hills. But this it does by moving along the surface of the ground. It has produced disease at an elevation of from three to five hundred feet above its source. Of this Dr. Ferguson has given us several instances; and others equally conclusive might be adduced. A current of air may bear it up an ascent, in the same manner as along a level surface.

Can marsh miasm travel far along the surface of water? We have what I deem satisfactory reasons to believe that it cannot. It has been already mentioned, that it cannot reach the crew of a ship lying at anchor, but a cable's length from the shore where it is generated. To prove this, facts innumerable might be adduced. The histories of commerce and war, in the West Indies, and other warm and sickly climates, abound in them.

Similar facts may be collected from the history of yellow fever, in our own country. During the prevalence of that disease in Philadelphia, many individuals, and several whole families, are known to have retreated to vessels lying not more than from two hundred to two hundred and fifty yards from the wharves, and to have remained healthy. In New York and Baltimore like instances have occurred. The intervention, then, of a narrow river, provided its own alluvion did not produce miasm, would afford ample protection from the march of that poison.

When persons are necessarily exposed to marsh miasm, can any effectual means be adopted to secure them from its deleterious effects?

I know of none. Camphor, vinegar, tobacco, and all other volatile and odorous substances, so generally resorted to, are useless. The employment of them is founded in ignorance, and ought to be rejected, as a species of quackery. It is as much the result of antiquated superstition, as amulets against witchcraft, or a belief in the performance of miracles by the relicks of Saints. Nor have I much respect for the purifying process by

acid fumigation and whitewashing. The best that can be said of it is, that it removes one kind of adulteration by another not so bad. Nor does it always do even that. The only real purifiers of foul ships, hospitals, and sick-rooms, are clean water, good soap, and free ventilation. Let these be competently employed, and the danger of infection will be but a name.

Not a little has been said about the diet, drink, and general regimen best calculated to protect those who are exposed to febrile miasm. Some recommend a very moderate diet, consisting chiefly, if not exclusively, of vegetables, with water as the only drink. The object of this is stated to be, so to purify the blood, that the miasm may find in it no suitable matter on which to act. Others urge the propriety of a free, if not a full diet of animal food, with a liberal use of wine, or some other stimulating beverage, with a view, as they express it, to "live above the fever."

In the devising of neither of these plans of prophylaxis, does reason or experience appear to have been consulted. If a person exposed to the action of marsh miasm escape its effects, it is in consequence of the successful resistance which his constitution makes to it. But the stronger in constitution, and the more firmly settled in health the individual is, the more powerful and unconquerable will be that resistance.

How, then, is this soundness and vigour of health to be maintained? The answer is plain. Not by any sudden and great change in diet and drink. Such a measure must always unsettle the constitution, and, by deranging its balance, weaken it. A sudden relinquishment of old habits is always hazardous. Hence it is particularly so, during the prevalence of an epidemic, when the powers of the system should be in full vigour and harmony with each other, that they may resist, with the more certain success, the efforts of a common enemy. The rule of wisdom appears to be as follows. Let those who are exposed to a febrile miasm, persevere steadily in their usual course of diet, drink, and regimen, provided they have found it, by experience, to agree with them; I mean, provided it has secured to them the greatest amount of health, strength, and general com-

fort, of which they are susceptible. For, in proportion to these will be the resistance of their systems to the action of the poison. Any thing that may in the least derange their health, or weaken their powers, increases necessarily their liability to disease. It is like the mistake of a military commander, who changes imprudently his order of battle, in the face of an enemy ready to take advantage of it; a movement which almost certainly invites disaster. During the prevalence of an epidemic, then, let those whose habit it is to eat animal food and drink wine or spirits in moderation, continue to do so; while those whose diet has been vegetable, and their potation aqueous, should persevere in them; provided, I say, they have found them salutary. Entire health and strength being the objects at which they should aim, the means by which they have found them to be best secured, they should perseveringly employ.

During epidemic periods some physicians direct the frequent use of purgatives, tonics, or some other medicinal articles, as preventives of disease. This is also an injudicious practice. Provided health be sound, such a course is not only unnecessary, but injurious. The end of medicines is not to preserve health, when it is perfect, but to restore it when it is lost. The only genuine preservatives of health are suitable diet and drink, judiciously indulged in, and a steady perseverance in a well directed regimen, including suitable clothing. And the clothing should be such as may best maintain the action of the skin. By the use of active medicinal substances perfect health is necessarily deranged. During an epidemic, then, let them never be employed, unless called for by an evident commencement of indisposition; and then let them be taken without loss of time. Should constipation of the bowels occur, let it be removed; because it indicates derangement of an important function. But active purging, however useful, as a means of cure, is not a safe preventive. Such appears to be true prophylaxis. The very anxiety attendant on the frequent use of medicine, for the prevention of a prevailing disease, deranges health, and weakens the constitutional resistance, which would

otherwise be made. Hence timid and pains-taking individuals more frequently suffer, than those who, free from dread, pursue their usual occupations, and adhere to their habitual customs. It is to be understood, however, that, during the prevalence of an epidemic fever, fatigue and all violent exciting causes should be carefully avoided.

A few further remarks on the miasm of typhus fever shall close this memoir. There is perhaps no other febrile poison, respecting which so great a change of opinion has occurred, of late, as that which has taken place, with regard to this.

It is not long since the belief in the specific and contagious nature of typhus, was almost as universal as in that of small-pox. A denial of the correctness of this belief was not only denounced, as medical heresy, but openly scoffed at, as medical folly. The author of this article has good reason to know that he speaks truth, because, having been himself a very early dissenter from what was then deemed orthodoxy, on the subject, he has been often assailed in the manner here indicated.

But very different, with many physicians, is the present state of opinion. Those who were formerly strenuous advocates for the contagious nature of typhus fever, have not only renounced that hypothesis, but contend now, that the complaint has a common origin with intermittents and remittents; and, of course, with yellow fever and pestis vera. Such is the vibration from one extreme to the other, which, in the moral and intellectual, as well as in the physical world, so often takes place.

In the correctness of the opinion just referred to, I yet want facts to induce me to concur. That it is plausible and ably defended, cannot be denied. But it is not yet definitively established. Although long persuaded that typhus fever is the product of a miasm generated by changes in dead matter, and not by morbid secretion, I have no solid ground to believe its miasm to be the same with that which gives rise to intermittents and remittents. On the contrary, I am still inclined to consider the two poisons different. My opinion to this effect rests chiefly on the following grounds. It will be understood that I am about to speak of typhus, not as it is sometimes re-

ported to us from abroad, but as it always appears in our own country. And I am induced to believe that the diseases are not the same. I mean, that they do not arise from the same cause.

1. For reasons already given, the miasm of intermittents appears to be the offspring chiefly, if not entirely, of dead vegetable matter. But that of typhus springs from animal matter perhaps alone. As formerly stated, it seems to be the product exclusively, of changes that take place in certain secretions of the human body, after their escape from the secreting organs. All circumstances, connected immediately with its production, favour this belief. In particular, it is frequently generated in places where no vegetable matter can be found; while the poison of intermittents is produced in abundance where no animal matter appears, but never without vegetable. It may be added, that typhus fever often makes its appearance where there is no decomposable matter but human exhalations.

2. The effects of the two poisons on the system of man are very dissimilar. Few febrile diseases differ more widely from each other, in their characteristic phenomena, than typhus and an intermittent. Neither in their access, type, progress, symptoms, nor duration, have they any resemblance. They have, at least in these respects, no more of resemblance, than every febrile affection has to every other. The same arguments, therefore, which would prove them identical, would do the same, as relates to every other form of fever. If they be the same, fever is a unit. Small-pox, measles, scarlatina, and pestis vera, are, in origin, identical. But such a conclusion would be at war with just reasoning. Were it true, philosophers might no longer contend, that similar causes produce similar effects.

3. No less dissimilar are the situations in which the two miasms are produced. That of intermittents has its birth in the open air. Nor does it matter, in the slightest degree, whether the atmosphere be calm, or agitated by wind. But the miasm of typhus requires, for its production, close rooms and a stagnant atmosphere. In the open air it is never generated. Free ventilation is as fatal to it, as the north is to the plantain, or a

tropical sky to the reindeer or the martin. I allude only to local typhus, usually called jail and hospital fever. With epidemic typhus, such as our late peripneumonia typhoides, the case is different. The poison which produces it, is bred in open places, and bears ventilation. But no one will contend that it is the same with that which is the cause of intermitting fever. Such an allegation would be worse than hypothesis. It could scarcely escape the name of lunacy.

4. Tropical climates are peculiarly the birth-place of the miasm of bilious fever. But under their influence typhus miasm is unknown. It is, at least, a very rare production. It is the native of a higher latitude, and a cooler climate. It is generated, moreover, chiefly during the winter and spring. But, in temperate climates, those, for example, in which alone typhus fever ever prevails, bilious miasm is the product almost entirely of summer and autumn.

5. There is reason to believe, that typhus miasm can adhere to the body and clothes of an individual, and being, in this way, carried to a distance, escape from him, and generate disease in other persons. And the individual, from whose secretions the poison is formed, may still retain his health. Thus, prisoners taken from their dungeons to trial, themselves free from typhus, have, by the miasm carried along with them, produced that complaint in those whom they approached.

To say nothing of the reported occurrences at the Black Assizes and the Old Bailey, other facts of similar import may be adduced. If my own authority may be admitted in support of my position, I will relate one which I witnessed myself.

A criminal, who had been long confined in a small, foul, and badly ventilated dungeon, was about to be brought to trial. His council wishing to converse with him, but unwilling to enter a place so offensive, had him brought into an adjoining apartment. During this conference, he was assailed by a noisome odour, from the culprit's body, which produced, once or twice, a slight degree of nausea. In a few days afterwards, he sustained a very violent attack of typhus fever, from which he recovered with great difficulty. The place, at the time, was free

from disease; nor had the prisoner himself been sick. The poison issuing from his person and clothes, was the only cause, to which the disease of his counsel could be referred.

But, of the miasm of bilious fever, the same is not true. No instance can be adduced, where it has been thus carried, and thus communicated. Yet being so much more commonly and abundantly generated than the miasm of typhus, such instances ought to be numerous, were it capable of producing them. To the instances of typhus thus produced, they should be as ten thousand to one.

6. The miasm of typhus is much more destructible than that of intermittents. It cannot, as already represented, be generated in the open air. Nor can it, when formed, bear the action of the atmosphere, in its common state of purity, without losing its deleterious character. It cannot, therefore, pass through any considerable body of atmospheric air, and produce disease. It must act near its source, or not at all.

But very different, in these respects, is the character of marsh miasm. It is not immediately neutralized, or in any way deprived of its deleterious qualities, by admixture with unadulterated atmospherical air. It can pass to a considerable distance from its source, and still retain its virulence. Hence it propagates disease much more extensively than the miasm of typhus. In fine, typhus miasm depends on man for its production; and, that it may generate disease, its subjects must be shut up within a narrow compass. Hence its proper sphere of action is in cities, towns, crowded dwellings, and other confined places. Its product, therefore, as already intimated, has been called jail, ship, and hospital fever. But, as respects its generation, the miasm of intermittents is independent of man, and attacks him, wherever it finds him; alone or in crowds; in the city or the country. It is produced every where, and the human family is every where its prey. For these reasons, I cannot believe the poisons identical.

It has been remarked, that marsh miasm produces disease in domestic animals. Of the miasm of typhus the same is true. But the poison is generated by the animals themselves; I mean

by deleterious changes in their exhalations. Confine horses, cows, and sheep, too long, and in crowds, in badly ventilated stables and folds, and they will suffer from typhus. This is perhaps more especially true of the latter animals. The vulgar name of the disease which attacks them is the "*rot.*" But it is a true malignant fever, of a typhus character. Nor is its source doubtful. Ventilation and cleanliness prevent its production, and extinguish it after it has been produced.

It will be observed, I repeat, that the form of human typhus here referred to, is that which arises from local sources. Although it is probably produced most readily, and marked with most malignity, when the general constitution of the atmosphere is unfriendly to health, yet it may be produced when no deleterious constitution exists. It is then purely local, the result exclusively of a poison generated by chemical changes in human exhalations. This form of fever I say, is specifically different from intermittents and remittents. It can neither be converted into them, nor they into it. Remittents do, indeed, at an advanced period, and under improper treatment, pass into what is denominated a *typhoid state.* But they do not pass into *genuine typhus*, any more than into *small-pox.* All violent and protracted febrile affections, whatever may be their original type and character, run toward the close, into typhoid action. This is very often the case with small-pox and scarlatina, and not unfrequently with measles. But those complaints are not typhus. No febrile disease changes its nature. It does not begin as one kind of fever, and terminate as another. Marked by the occurrence of a few new symptoms, it is, from beginning to end, specifically the same. New symptoms appear in many cases of small-pox, as well as of typhus. But it is small-pox still. And typhus has a character as specific and unchangeable as it has.

Of the origin of *epidemic* typhus, it is not my purpose to speak. That which, some years ago, passed over the United States, under the names of *typhus syncopalis*, *peripneumonia typhoides*, and *cold plague*, had no manifest dependence on place, season, or any of the sensible qualities of the atmosphere. It prevailed at all

times, in all situations, and during every description of weather. It was as completely atmospherical, as influenza or scarlet fever. Nor was it less different than they are from intermittents and remittents. Its origin, moreover, was equally obscure. To pronounce it the product of some terrene exhalation, would be to utter a mere conjecture. And yet the science of medicine cannot, at present, offer any thing better.

From having been an exclusive contagionist, Dr. Armstrong now derives from the same *malaria* which produces intermittents and remittents, the epidemic typhus, which lately prevailed in England and Ireland. In proof of his opinion, he adduces the prevalence of those diseases chiefly in the same places, and especially *their convertibility into each other.* He asserts that he has seen intermittents and remittents changed into typhus, and the reverse.

If this be true, they are but modifications of the same fever, and are, therefore, the product of the same miasm. But, of intermittents and remittents, and the typhus fever of the United States, whether epidemic or local, I repeat, it is not true. Those complaints are not confined to the same places; nor are they convertible into each other. They are not even most prevalent at the same season of the year. In fine, they are dissimilar in all points essential to character; nor do they seem to differ more in any respect, than in the causes which produce them.

ART. II.—*An Essay on the Absorbent System and its Functions.* By JAMES CONQUEST CROSS, M. D. of Fayette county, Kentucky.

[CONTINUED FROM PAGE 364.]

FUNCTIONS of the Absorbent System.—The investigation prosecuted thus far has been almost exclusively anatomical. Besides an exhibition of the nature and extent of the absorbent system as a whole, and of its several divisions as distinct and separate integral parts, it is true, we have given an ample and detailed account of its properties, but these do not constitute its functions, nor are they capable, individually, of exercising any of the processes of the animal economy. It is only by the exertion of the cumulative energies of several of them, that any of the offices of life can be properly performed.

In the study, therefore, of the separate and independent functions of the absorbent system we shall immediately engage. In this disquisition, we design pursuing the same course so rigidly adhered to in the anatomical investigation of the different distinct divisions of this system. In accordance with this arrangement, the

Functions of the Lymphatic absorbo-exhalents must come first under consideration. That order of vessels of this division of the absorbent system, which originates from the villi of the small bowels but chiefly from those of the duodenum, was designed unquestionably for the absorption of chyme, which, after the process of digestion has terminated, generally exists in great abundance. Although this proposition is almost intuitively evident, it has been assailed so fiercely by a distinguished and authoritative physiologist, as to have kept public opinion, held for some time, in perturbed suspense. Unsettled, however, as opinion decidedly was for a while, so victoriously have the peculiar sentiments of M. Magendie been combatted by some of the ablest disciples of Hunter, that it surely would be a

work eminently supererogatory to adduce evidence to prove the truth of a position, which doubtless, receives the unqualified assent of every sound and enlightened physiologist. Taking it for granted, therefore, that the particular office of the vessels which are usually known by the denomination of lacteals, is to remove chyme from the duodenum, we shall pass on without additional comment, to the examination of the functions which devolve upon the only remaining portion of the class of absorbents at present under consideration.

Upon this subject there is no discrepancy of opinion: among physiologists there is, here at least, an unanimity, perfectly undisturbed, and unfluctuating. They all concur in admitting, that they absorb a fluid called lymph, and that after its accumulation in the thoracic duct, it is exhaled into the venous circulation. But certainty, at this point, ceases: here conjecture commences her reign. The source of lymph has given rise to a multiplicity of speculative opinions, all of which, we are constrained to consider gratuitous and unfounded. There are two, however, that possess in our humble belief, the equivocal merit of plausible hypotheses.

By the espousers of one of these hypotheses it is contended, that lymph is made up of the debris of the solids and fluids, which enter into the composition of the animal body, or rather of those exhausted substances, which have become old, impoverished, and consequently unfit to remain in it. Coming from such a diversity of sources, lymph must of necessity be composed of a great variety of very different materials. For as all the solids as well as fluids, are made to contribute to its production, it is impossible for lymph to exhibit an uniformity of character. But this is in direct opposition to its most conspicuous characteristic attribute. Thus, by M. Ribes it is asserted, that while foreign substances such as oil, pus, &c. are frequently found in the veins, no foreign article is ever discovered in admixture with the lymph. By M. Magendie also it is remarked, that "the lymph has always been found to possess the same sensible qualities in all parts of the body." These statements are manifestly irreconcileable with the assigned origin of

lymph. To invalidate this opinion, however, it may be urged. that by Mascagni and even by Magendie himself, oil has been found with lymph in a state of admixture. In health this has never been observed in pure lymph. It should moreover be recollected, that the assertions of the distinguished individuals alluded to, were predicated upon observations made with lymph and chyle mixed together. This being the case, it is not surprising that they should have discovered in the mixture, occasional particles of an oily or fatty substance; for it has been ascertained by M M. Halle and Dupuytren, that such an appearance is very commonly exhibited by chyle. Let it be distinctly understood, that we contend for the uniformity of lymph only under ordinary circumstances. Substances essentially unlike lymph in the lymphatics, when abnormal action invades them, is perfectly consistent with the opinion maintained by us, and is a phenomenon not altogether of rare occurrence.

Chyle is a fluid exclusively nutritive. Is it, therefore, reasonable to suppose, that nature designed its adulteration, by incorporating it with a fluid, which, if at all alimentary, must unavoidably possess that attribute in a very low degree? This must be the inevitable result if lymph really has its source in the debris of the solids. How clearly inconsistent is this with the purpose most emphatically revealed by the vessels which absorb the chyme? As has been already remarked, the researches of M M. Gmelin and Tiedemann, have satisfactorily ascertained the fact, that of all the vessels of the absorbent system, the chyliferous are those which refuse most obstinately to take up foreign substances, or to accomplish what they have called *accidental absorption.* Is it possible for this fact to be misinterpreted? Is it not manifest, that its special design is to prevent the chyle from admixture with any improper substances, that may be in the intestinal tube? But if lymph is composed of the old and exhausted elements of the organs, how exceedingly preposterous, to erect an almost insurmountable barrier at the radicules of the chyliferous vessels to prevent the absorption of unfit articles, and immediately afterwards to incorporate the fluid absorbed, with a compound necessarily heterogeneous?

It is asserted by MM. Chaussier and Adelon, and they express nothing more than a prevalent opinion, that no substance can enter into the composition of lymph but that which is eminently nutritive. Is it not, therefore, supremely preposterous to contend, that lymph has it source in the debris of the organized solids? To demonstrate that the materials which constitute the supposed origin of lymph, are in every respect totally incapacitated to yield nourishment to the living body, is unconnected with difficulty. Is it not intuitively evident, that these substances would not be removed if their nature was such as to render them suitable ingredients in the structure of the body? Their unfitness is the exclusive reason why interstitial absorption is exerted for their removal. Is it not, therefore, a cause of great surprise, that it should ever have been supposed by any one, that materials which have become by age unfit to remain in the structure of the organs, should be removed for no other purpose than to produce an eminently alimentary fluid designed to nourish the body? If this were true, the animal economy would possess within itself abundant means of nourishment, which is disproved by the melancholy fact, that death is frequently the result of inanition. Is it possible, moreover, to assign a satisfactory reason for the institution of the process of interstitial absorption, if the results of it are to be thrown directly into the sanguineous circulation, and from thence redeposited in the parenchymatous tissues of the organs? Thus viewed, the purposes of the economy are so enigmatically expressed, as to be absolutely unintelligible. Contemplated then upon its most favourable side, the deduction which this hypothesis authorizes, is, that the materials of nutrition, which have become exhausted by remaining a certain time in the structure of the organs, are, by passing through the lymphatic absorbo-exhalents into the sanguiferous circulation, completely regenerated, or put in full posession of all the attributes which constituted their original excellence. An inference subversive of every principle of sound philosophy, and at war with every thing confessedly true in physiology.

Lymph is a vital fluid, but the result of interstitial absorption

is necessarily destitute of the living principle. In the structure of the organs, that, which is to constitute their immediate debris, is certainly in possession of vitality, but it is a fact, the truth of which cannot be questioned, that they cannot be removed until this principle in them is extinguished. No more conclusive evidence in demonstration of this can be required, than, that no organ can be absorbed until its texture is destroyed—until its organization is broken down—until, in fact, it becomes completely fluid. Is it not evident, therefore, that as the life of a tissue arises exclusively from its organization, the former must be extinguished as soon as the latter is destroyed. In a state of aggregation, it is impossible for any solid substance to enter an absorbent vessel: transmutation into the state of a fluid, is an indispensable preliminary step.

This is the real character of lymph, if the source to which it has been ascribed, is founded in truth. How exceedingly nugatory and idle was it therefore, in nature to guard with such sedulous care the radicules of the chyliferous vesels, when the chyme is scarcely converted into chyle, before it is intimately commingled with the lymph, a fluid which is not only destitute of the principle of life, but a compound eminently heterogeneous, and possessing consequently all the attributes of a foreign substance. But, that nature never incorporates fluids thus dissimilar, is proved by the fact, that it would be the most direct and effectual means, that could possibly be adopted, to counteract all the efforts made by the organs of assimilation.

The hypothesis which we are combatting, renders it necessary to ascribe to the same organ, the execution of two functions, that are not only unlike, but, which are in a state of direct antagonism. Thus it is through the thoracic duct, that the materials of recuperation are carried into the circulation, and it is through the same channel, that the proceeds of insterstitial absorption pass before they can be exposed to the action of the organs of elimination.

These considerations induce us to conclude, that the hypothesis, which we have been examining, is altogether destitute of a substantial foundation.

The other hypothesis is predicated upon the belief, that nature is very prodigal in supplying nutriment to the different organs; that it is furnished in greater abundance than their absolute necessities demand. In consequence of this, there is a residue, which the lymphatic absorbo-exhalents absorb, and it is from this source that lymph derives its origin. The only advantage which this theory has over that the absurdity of which has beeen sufficiently shown, consists in the nearer approximation which it makes to the real nature of lymph. In other respects, it is, if possible, more manifestly erroneous. It is founded upon a gratuitous supposition, infinitely disgraceful to nature as an artist. It asserts her inability properly to adapt means to the accomplishment of specific purposes; an attribute for which she is more distinguished than any other. To assert that it was necessary to create an order of vessels, in order to shield the system from the disasters to which it is daily exposed, in consequence of the awkward and blundering manner in which the function of nutrition is performed, is preferring a charge, which is clearly unfounded, and which it is impossible for the most ingenious sophistry to render plausible. It leads to conclusions which are in direct contravention to the most familiar facts. Is it true, that in those who labour under excessive obesity the lymphatic absorbo-exhalents are in a state of paralysis? Or, in those who labour under corporeal extenuation, is it established by researches in pathological anatomy, that they have made abnormal exertion?

If it be true, that lymph is nothing more than the residue of superabundant nutrition it should never be found in animals that have fasted several days. This is not the fact, however, for it is in similar circumstances that that fluid can be easily obtained. The author has repeatedly discovered lymph in the thoracic duct of dogs after fasting had continued as many as four days. From which it appears that it is impossible to consider it as proceeding from the residue of nutrition.

Much research and considerable reflection have induced us to consider lymph as consisting of chyle absorbed from the ca-

pillary blood-vessels to be re-elaborated by the lymphatic absorbo-exhalents.

In the preceding pages, it has been sufficiently demonstrated, that this division of the absorbent system does in part originate from the minute blood-vessels: it cannot, therefore, at this time be necessary to detail evidence of the fact. Without, however, burdening our subject with unnecessary proof, it may not be unimportant to inform the reader, that this communication is recognized and admitted by the ablest authorities living. Thus by M. Magendie it is remarked that "It often happens, that an injection forced into an artery, passes into the lymphatics of the part to which it is distributed." By M. Adelon it is said that "dans ces systemes capillaires il y a communication facile entre les diverses especes de vaisseaux qui les forment, entre les arteres et les veines, les arteres et les lymphatiques, les veines et les lymphatiques meme."

According to the opinion above expressed, there can be detected, no radical or essential difference between lymph, chyle and blood. Inconsiderable shades of difference are certainly obvious, but the resemblance between them is so striking, that it is impossible not to consider them fundamentally identical.

Formerly there was considerable discrepancy of opinion with regard to the specific nature of chyle, but at present, no doubt exists upon the subject. By Lower and Brunner it was considered identical with milk; by Wepfer, Pecquet and Moro a species of cream; and by Lister of an oleaginous nature. We are, however, particularly indebted to the labours of Halle, Dupuytren, Magendie, Vauquelin and Marcet for a knowledge of its real nature. From these distinguished individuals we learn, that chyle has a sweetish and slightly saltish taste; exhales an odour like that of human semen; of a pinkish colour; congulates or rather assumes a short time after exposure to the action of atmospheric air a gelatinous form not unlike a coagulum. Beneath this there is another perfectly liquid portion, which is elevated above the coagulum, whenever its circumference is detatched from the inner surface of the vessel in which it is contained. It thus appears, that chyle is divisible into two distinct parts.

The liquid portion is very clear and resembles the serum of the blood. That which coagulates, is solid, dense and white. This division of chyle M M. Halle and Dupuytren consider false and unnatural. They contend, that it is composed of or is divisible into no less than three separate superposed parts. The primitive stratum, if we may be allowed the expression, they inform us is composed of fibrin and red colouring matter, and is consequently solid; the second is liquid and is analogous to the serum of the blood; and that upon the surface, although exceedingly thin and delicate, is opaque, of a white colour, and of a nature entirely oleaginous.

By a total fast, for the space of several days, lymph may be obtained nearly pure. There is then, but little, if any chyle in the thoracic duct. Lymph thus procured, we are informed by M M. Chevreuil and Magendie, has a pinkish colour; an odour manifestly analogous to human semen; and a taste decidedly sweetish and saline. Usually it does not remain long in a fluid state. Rest and exposure to the action of air, soon transform it into a solid mass. In a state of coagulation, the pinkish hue is much more distinctly pronounced, and then there are in it a multitude of reddish filaments arranged in irregular *arborisations* not unlike the small vessels found in the organized tissues. An attentive examination of coagulated lymph will show distinctly that it consists of two separate and dissimilar parts. One is solid and the other liquid. If the former is separated from the latter, the latter also will become in great part solidified. Between the solid part of lymph and the coagulum of the blood, there is a striking resemblance. Like it, when exposed to the action of oxygen, the coagulum of lymph assumes a shining red colour, and when immersed into carbonic acid gas, it is transformed into a deep purple red.

The resemblance between the attributes of chyle and lymph, as above exhibited, is too striking, not to challenge particular attention. They are both discharged into the venous circulation; they are both highly nutritive; they have the same taste; the same odour; the same colour; they both coagulate; they separate into two parts—one liquid and the other solid. M M.

Leurent and Lassaigne, have analyzed the chyle and lymph of numerous animals, and they inform us, that chemistry possesses no means of detecting an essential difference between them. From M M. Halle and Dupuytren we learn, t at they observed upon the chylous coagulum a fatty or oily substance, which they had never seen upon lymph. Although this evidently discloses an unimportant difference, it does not furnish us with the means of distinguishing chyle from lymph. Its appearance is occasional and accidental only—not universal, and its occurrence is ascribable exclusively to the particular kind of aliment upon which the animal has subsisted. While it occurs freqently in carnivorous, it is never present in animals entirely herbivorous.

There is very great analogy between chyle and blood. The separation of the former into two distinct parts, resembles very much the spontaneous separation of the latter into crassamentum and serum. The chyle, likewise, resembles the blood in the nature of its salts. The properties of the liquid part of chyle are very similar to those of the serum of the blood. The chyle is fibrinous, but its fibrine is less elastic, and is more completely and promptly soluble in caustic potash than that of the blood. In the latter too, fibrine exists in greater abundance, and is of greater specific gravity. Between chyle and blood Dr. Bostock attempts to establish an essential difference upon the same basis precisely as attempted by M M. Halle and Dupuytren. By Dr. Bostock it is said, that chyle contains a quantity of an oily or fatty matter, "an ingredient which is only occasionally found in the blood." It should have been recollected, however, before he attempted to establish a distinction upon so frail and uncertain a foundation, that this "ingredient" is only of occasional occurrence in chyle. Thus it is said by Dr. Marcet, that oil or fat is found in animal but never in vegetable chyle.

The attributes of chyle and blood, both in a chemical and physiological point of view, are such, therefore, as to authorize us in considering them as strongly resembling though not identical fluids. Between them, however, there are those who contend for a still more close analogy. Thus M M. Parmen-

tier and Deyeux were disposed to believe, that the only difference which exists between chyle and blood, consists in the colouring matter of the latter.

Connected with this enquiry, it is important to ascertain, if there are any points of analogy between the lymph and blood. Having already demonstrated, that there is considerable resemblance between chyle and lymph, as well as between chyle and blood, reasoning *a priori*, we are authorized to assert, that a similar analogy will be found between lymph and blood.

It has been already remarked, that there is considerable likeness between the solid part of lymph and the coagulum of blood, and, that the same effects are produced in the former by the action of oxygen and carbonic acid gas as in the latter. Like blood, says Mascagni, lymph in the course of from seven to ten minutes separates into two parts. In the midst of the more abundant serous portion, a fibrous coagulum floats, which, by contracting, is formed into a small cake. From which he concludes, that lymph consists for the greatest part of serum and that fibrin constitutes its least part. The fact of lymph separating into two parts cannot be doubted, but, that it consists for the greatest part of serum is rendered very questionable by Sœmmering, and, that fibrin constitutes its least part is disproved by the researches of M. Chevreuil and others. By M. Richerand, it is said, that the lymph in man and warm-blooded animals, appears in every respect similar to the fluid which is contained in the vessels of white-blooded animals. By Mr. James Wilson, the author of a Croonean Lecture, it is said, that in all probability, the lymph is something more than the fluid absorbed from the surfaces upon which the arteries had deposited it to lubricate them; for such fluid does not coagulate in a heat less than 160°, whereas the lymph coagulates upon exposure only, and in so doing it greatly resembles the coagulable lymph of the blood. We are told by M. Magendie, that in some cases, there is so little difference between the lymph and the blood, that it will be difficult to distinguish them.

These statements, particularly when associated with the testimony of MM. Leurent and Lassaigne, who say it is impos-

sible to discover an essential difference between chyle and lymph, and with that of M. M. Parmentier and Deyeux, who inform us, that they consider the only difference between blood and chyle to consist in the colouring matter of the former, must satisfactorily prove, that the difference between lymph and blood is of necessity very inconsiderable.

Shades of difference doubtless exist between chyle, lymph and blood, but they are of such a character as to convince us, that they proceed exclusively from different degrees of elaboration. If the hypothesis we are attempting to establish is true, thorough investigation must prove, that blood is a more vital or more completely elaborated fluid than lymph, and lymph more so than chyle. That chyle is more so than chyme has already been proved.

Thus, fibrin is more abundant and of greater specific gravity and is more elastic in blood than in lymph. Blood coagulates sooner than lymph and its colour is more decidedly affected by oxygen and carbonic acid gas. That lymph is more amply supplied with fibrin than chyle, is proved by the unimpeachable testimony of M M. Gmelin and Tiedemann. These individuals inform us, that fibrin abounds in the lymph of the lower extremeties, and that it is more abundant in the chyle after the addition of the lymph from the spleen. The legitimate conclusion from which is, that lymph is richer in fibrin than chyle. Actual experiment has proved to us, that lymph sooner coagulates and that its colour is sooner and more manifestly affected by the action of oxygen and carbonic acid gas than chyle.

In accordance with the opinion here avowed, chyle should not be so instantaneously, as is generally supposed, incorporated with the blood, as to prevent under any circumstances a distinction between the former and latter. That the prevailing opinion is false and unfounded a little examination will convince the reader.

In numerous cases recorded by Ruysch, Glisson, Haller and Morgagni of the discovery of a chyle-like fluid mixed with the blood, it is remarkable, that there was generally some affection of the heart or embarrassment of respiration, either idiopathic

or sympathetic. In these instances sanguification was doubtless carried on imperfectly. We are told by Bilsius, an old author quoted by Baron Haller, that he had seen a grey chyle in the blood of the mesenteric veins. In the same vessels, Sœmmering is of opinion, that he had seen something similar. The existence of chyle in the blood is admitted by Mr. Cruikshank.

By Lower it is contended, that chyle is not immediately assimilated with the mass of blood but that it floats in it for some time in its original state. In support of this opinion a most remarkable case is adduced. "A maid, after eating a good breakfast about seven in the morning, was let blood about eleven the same day in her foot. The first blood was received in a porringer, and within a little while it turned very white. The last blood was received in a saucer, which turned white immediately, like the white of a custard. Within five or six hours after, I chanced to see both; and that in the porringer was half blood and half chyle, swimming upon it like a serum as white as milk; and that in the saucer all chyle without the least appearance of a drop of blood. And when we heated them distinctly over a gentle fire, they both hardened as the white of an egg when it is heated, or just as the serum of the blood doth with heating but far more white. The maid was then in good health, and only let blood because she never had her courses, yet of a very florid clear complexion."

By Blumenbach, we are informed, that he has witnessed the appearance of chyle in the blood drawn after digestion. By Dr. Rush, it is said, that chyle is found in the blood after it has passed through the lungs. In a number of experiments performed upon dogs by Dr. Hutchinson, this fact was fully established. We are told by Professor Dumas, that when a vein is opened in an animal that has eaten plentifully, evident traces of chyle may be detected upon the surface of the blood, after the process of digestion has terminated.

From evidence multiplied and unquestionable we are constrained to admit, that chyle is not immediately amalgamated and confounded with the blood;—that passing through the lungs once only, is not sufficient to accomplish this object. Indeed, we have satisfactory reason to believe, that in its formal state

as pure chyle, it has not yet acquired those properties by which it is rendered a proper stimulus to the organs with which it comes in contact; for we are assured by Professor Martini, that if a more abundant quantity of chyle than is customary, enters the circulation, abnormal action is sure to be the consequence.

In certain morbid conditions, pure blood will be absorbed by the lymphatic absorbo-exhalents instead of chyle. Such phenomena may be adduced as legitimate proof of the hypothesis here defended. Thus it has been ascertained by Mascagni, that the lymphatics of the lungs and peritoneum are frequently filled with blood in animals that have died in consequence of pulmonary or abdominal hæmorrhage. From which he concluded, that these vessels absorbed the fluid with which they were distended. M. Amussat, exhibited to the Academie Royale, a morbid preparation, taken from the body of a person who had died of small-pox. In this subject the attempt to inject the lymphatic absorbo-exhalents had been made in vain; and the obstacle was found to consist in the lymphatic absorbo-exhalents of the abdomen, and even the thoracic duct itself, being loaded with blood. The last-mentioned phenomenon, it is remarked, appears to have been observed by others; but is not indicative of an inflammatory state of the vessels themselves; which, after removing the blood, (absorbed by them unquestionably,) exhibited their natural physiological appearance. In animals as well as in men M. Magendie has frequently seen these vessels filled with blood, where there had been no extravasation of this fluid.

By M. Magendie, it is stated, that as abstinence is protracted, lymph assumes progressively a deeper red, and at the expiration of eight days it is as red as blood. And Sœmmering long ago said "Inter epulas atque pocula, novi per vasa absorbentia canalis intestinalis humores sanguinem subire incipiunt." The reason of this is obvious. After the animal has fasted for some time chyle cannot be as usual periodically supplied. The necessary consequence of which is, the lymphatic absorbo-exhalents must either cease to act altogether, or they must act upon the blood itself. This is facilitated by the modification which the elective sensibility of these vessels must sustain from a par-

ticipation in the impression made upon the system universally by protracted abstinence. Thus, in an abnormal state, they absorb blood, but while they retain their physiological integrity, they obstinately refuse to do it.

It is conjectured by M M. Tiedemann and Gmelin, that the red-colouring matter of chyle, is *secreted* from the arterial blood by the mesenteric glands and the spleen. In support of which it is said, that chyle is perfectly white before it has passed through the glands of the mesentery, but that it reddens especially after the reddish lymph from the spleen is mixed with it. This hypothesis is wholly without foundation. The premises are doubtless sound; but the conclusion is plainly unauthorized. We have no evidence to prove, that the lymph of other parts is not as red as that from the spleen. Indeed, there is every reason to believe that this attribute is preserved by the lymph throughout the system. It is true, that the lymph from the spleen is a little more abundant than from most other parts, and this renders it more easy to minutely notice its peculiarities. To admit that the lymph from the spleen only is reddish, or that it is more so than that of other parts, would be an admission in direct oppugnancy to a well established fact. The uniformity of character in the attributes of lymph throughout the system, a position sustained by irrefragable evidence, would be at once destroyed. It is altogether unnecessary to have recourse to the erroneous hypothesis of M M. Gmelin and Tiedemann, in order to explain the phenomenon from which it is deduced. It yields no inconsiderable support to the source to which we have ascribed lymph. How shall we explain its reddish colour, unless we attribute it to absorption by the lymphatic absorbo-exhalents from the capillary blood-vessels?

Thus the erroneousness of the most plausible hypotheses has been satisfactorily shown, and the following propositions fully established. First, that one portion of the class of vessels denominated lymphatic absorbo-exhalents originates from the capillary blood-vessels; secondly, that there is no fundamental difference between lymph and blood; thirdly, that the shades of difference are owing to their different degrees of elaboration;

and lastly, that chyle is not completely identified with the blood in passing once through the lungs. We shall conclude by remarking, that lymph is considered by M M. Adelon and Chaussier, a more perfectly animalized fluid than chyle, and that M. Dupuytren says its concrescibility is superior to that of venous and equal to that of arterial blood. This is considered by Hunter, an indubitable evidence of vitality.

Functions of the arterial absorbo-exhalents.—This class of vessels is the great elaborator of all the exhalations, excrementitial as well as the recrementitial, with the exception of the biliary exhalation of the liver; and with the exception of the hepatic and pulmonary exhalations, they all proceed from arterial blood. It is not our design, nor does it comport with the scope of this essay, to enter into a circumstantial account of all the exhalations specifically. With the exception of pulmonary exhalation, and the exhalation of animal heat, the remarks which we have to make must, chiefly be of a general character. Besides those already mentioned, more frequent reference will be made to nutrition than to any of the others, because it is better adapted to the purposes of general illustration.

Perhaps, it may be as well to advertise the reader, that we make no distinction between the exhalation of the elements of nutrition, such as phosphate of lime, fibrin, &c. and the exhalation of saliva, gastric juice, &c. because we consider them fundamentally the same. Upon the same principle all their particular phenomena are explicable. Of this principle we may be ignorant, but we are persuaded, that when it is ascertained, it will be found to be a unit, equally applicable to the whole as to a part of them.

Each exhalation, it is true, has its peculiarities, but this proceeds from a modification in the organization of the tissue by which it is produced, and this necessarily gives rise to a particular mode of sensibility. The merest tyro in anatomy can discover an obvious difference, between the organization of the kidney and liver, between the testicle and the mammæ, &c. Injections will succeed in some glands much better than

in others, and some exhalatory surfaces are much more frequently than others the seat of hæmorrhage.

The particular sensibility of glands, is disclosed in the manner in which they are affected by particular therapeutic agents. Thus mercury given in a certain way, will, excite the liver to an augmented exhalation of bile; and in another, it will stimulate the salivary glands to unwonted exertion. Cantharides will irritate the kidneys, and rich stimulating diet will increase the exhalation of semen.

These considerations afford abundant evidence, that each exhalatory tissue is distinguished by a particular organization; from which proceeds a peculiar mode of vitality, and the ultimate result of which is an exhalation in possession of distinct characterizing attributes. It is only in their sensible effects, it is true, that these various modifications of life can be appreciated; but they are as convincing evidence of the existence of an absolute difference in the minute structure of an organ, as if it could be submitted to the examination of our most unerring sense. That a material difference exists in the intimate texture of the nerves of the five external senses, is evinced by all their phenomena; but this has escaped the observation of the most powerful magnifiers. This must be, or the same cause precisely, can produce a multiplicity of exceedingly dissimilar effects.

Exhalation is a function exclusively vital. It is totally independent of any influence exercised by a physical or chemical cause. As to its particular mechanism, we can say nothing more than what was urged, when we treated of the function of absorption specifically. Like it, its essential nature is inscrutable. To us, it is a source of considerable satisfaction to know, that it is not dependent, either directly or indirectly, upon any agent not under the exclusive control of the vital principle. This it would appear is sufficiently obvious. To the judicious and impartial, at least, a *coup d'oeil* of the different hypotheses, that have been deduced from the principles of the physical sciences, together with a knowledge of their utter insufficiency, must constitute an *argumentum crucis* against all speculative inferences from premises that cannot be admitted. Thus, the first

theory of note among the moderns, is, that of fermentation, started and defended by Van Helmot and Vieussens; then came the filtering hypothesis, by which particles of a particular size or figure passed through pores of the same size and figure, and was supported by Charleton, Descartes, Borelli, and Verheyn; then the theory of attraction and repulsion, which numbered among its advocates the celebrated names of Winslow and Helvetius; then the thought struck Conor that the body was really alive, and he was disposed to ascribe exhalation to the agency of the nerves; but Boerhaave and Pitcairne, refused to admit it, and contended, that it was altogether owing to the velocity of the blood in the glands. The hypotheses of transudation, capillary attraction, &c. are equally gross, mechanical and unfounded.

The chemical theory, with its numerous modifications, has still many able partisans; and has been by several of them, defended in a masterly manner. The principles it inculcates are openly hostile to every view expressed in this memoir. To do ourselves justice, it will not do, to dispose of it in the summary manner we have the others of a physical nature. No apology can be required, for details upon this point, as the true interest of the science of life, imperiously demands, that chemistry should be driven from its borders. Although the paramount influence and importance of the vital principle must be universally acquiesced in, there still remains an interesting inquiry as to the structure and means employed. The difficulty, not to say impracticability of this investigation, may always prevent us from obtaining an unexceptionable theory of exhalation, yet, that physiologist, who acknowledges the unconditional and undivided influence of the vital principle, will always possess sounder and safer opinions, than the mere chemical philosopher. The false hypotheses of the former, have always, before the light of truth, promptly and innocently vanished in empty air, but those of the latter, before it has dawned upon them, have often succeeded in establishing practical principles, that have lead to consequences of the most deleterious character. Hecatombs, of individuals, have been wantonly sacrificed, at the

shrine of their unauthorized idolatry. These evils were first clearly seen by the illustrious George Ernest Stahl: who, though educated in the principles of Sylvius and of Willis, and the author of a brilliant chemical system, boldly denied the existence of any function in the living economy, that could be satisfactorily explained upon chemical principles.

Nor is it strange, that an hypothesis so radically and glaringly preposterous, as well as so pregnant with the most detrimental consequences, should have been condemned by the comprehensive and penetrating mind of Stahl. Not like most debateable questions of a purely speculative character, that have no standard to which we can appeal, in order accurately to measure their strength and soundness, the chemist himself has furnished us with a criterion, that will at once establish the truth or expose the falsehood of his hypothesis.

His investigations are exclusively analytical and synthetical. All substances that are chemically united, the votaries of the science of chemistry can separate and ascertain the nature of their immediate elements; and these elements they can again combine, from which will result a reproduction of the original substance. Thus, by analysis the chemist will ascertain that atmospheric air is composed of oxygen, azote and carbonic acid, and by synthesis he can reproduce it. In the same way, he can discover, that water is composed of oxygen and hydrogen, and in the same way he can reproduce them in the form of water. In every investigation, therefore, in which he fails to do this, he forfeits all just claim to the confidence and respect of the really scientific, if he does not unhesitatingly not only acknowledge the insufficiency, but the total inapplicability of the science of chemistry. When he has attempted to explain the functions of life, has he not signally failed in every effort that he has made? With the chemical elements of the body, has he ever succeeded in producing an organized tissue or an animal exhalation? When he has analyzed the blood by his reagents, why does he not recombine its principles, and thus reproduce true blood proper to circulate in the arteries and veins and yield nutriment to the system? Why can he not make semen out of a little

animal mucus and some of the phosphates? When gastric juice is not supplied in sufficient quantity by the stomach, why cannot we be accommodated by the apothecary? Let the chemist answer, why he cannot with a vesicatory, draw a blister upon a dead as well as a living leg. Such attempts would be ultra-extravagant: they would be justly considered as the distempered ravings of a maniac, and the chemist who could even dream of his ability to accomplish them, would be esteemed more eminently entitled to a birth in a mad-house than a place in the laboratory.

Should it, however, be alleged, and it must be upon untenable ground, that the standard to which we have appealed is unfair and exceptionable, the chemist cannot certainly refuse to answer in the affirmative, that, if exhalation is a chemical process, all the elements detected in the exhalations must of necessity have pre-existed in the gaseous or solid substances, that ministered to the sustentation of life. Here, his efforts to support his hypothesis, will prove equally abortive.

Under the control of the same principle, the functions are carried on in the vegetable as they are in the animal economy. Analogies deduced from the former may, therefore, be used as legitimate illustrations of the operations of the latter.

Thus plants are nourished by elementary substances so simple, that it is absolutely impossible for them to contain the immediate principles of nutrition. They will not only live but luxuriantly grow by the unaided use only of distilled water, air, caloric, and light, that are found upon analysis, to contain not an atom of many of the elements of which they are composed. Now, these principles are too simple, and too much unlike the composition of a vegetable, to authorize the supposition, that the nutriment of the latter, existed in a formal state in the former, and that its extraction is all that is necessary.

By the researches of Lampodius, Braconnot and Vauquelin, the fact has been abundantly established, that plants do yield, upon analysis, earthy constituents not a particle of which existed in the soil on which they grew. Cerialious plants, in particular, yield a considerable portion of siliceous earth, which is not

taken from the soil in that state, but is undoubtedly the result of the process of elaboration. This is proved by the fact, that when the soil in which they are raised is entirely divested of its silex, the plants upon analysis are found still to yield it. From very accurate experiments, Braconnot asserts, "that organic force, aided by solar light, developes in plants substances which have been regarded simple, such as earths, alkalies, metals, sulphur, phosphorus, carbon, perhaps azote." By M. Saussure it is remarked, that during growth plants acquire an additional quantity of carbon. According to the chemist Einhof, the cones of pines, that grew in a sterile and unproductive soil, and which did not contain a single atom of lime, yielded notwithstanding a calcareous earth.

In reference to the animal kingdom particularly, the evidence of the existence of chemical elements in the exhalations, that did not pre-exist in the food or air, is equally clear and satisfactory. Thus, there are animals capable of incorporating with the material structure of their organs the principles of air and water. Numerous examples are adduced by M. Rondelet, of marine animals, which, from the constitution of their organs, are obliged to find a suitable nourishment in water exclusively. He has himself seen a fish kept in a vessel filled with very pure water during the space of three years grow and flourish without the possibility of obtaining the means of subsistence from any other source but the water. Upon the authority of Dr. George Fordyce, we state, that the gold-fish was kept in water that was supposed to be pure, and by being merely supplied with air, it not only lived for many months but increased very considerably in size. By Redi, Mead, Vallisneri and Lacepede, we are told of reptiles that live exclusively upon water, and of this they consume a very inconsiderable quantity.

By Vauquelin, we are assured that in the fæces and egg-shells of fowls, that had been fed upon grain exclusively, he discovered a greater amount of carbonate of lime than the grain which they consumed would yield. In a train of well conducted experiments, performed by the accurate Dr. Prout, upon the composition of recent eggs, he ascertained the nature and quantity

of their elements, and then compared these with the composition of eggs at that stage of incubation when the chick is fully developed. From which it was discovered, that the animal had acquired a greater amount of earthy matter, than could be accounted for in any other way, but by ascribing its development to the elaborative exertions of the vital principle. He supposes, that the earthy matter of the bones of the chick "does not pre-exist in the recent egg." In another place, he further remarks, that there are "very good reasons for believing that the earthy matter is not derived from the shell." During the space of ten days a chicken was fed by M. Richerand, with corn mixed with silex exclusively. Upon examination he found in the excrement, considerably less of this earth, and, on the contrary, much more of the phosphate and carbonate of lime than was contained in the aliment.

As azote constitutes a considerable chemical element in the composition of the animal tissue, and as it also is found in most of the articles employed as aliment, the chemist has insisted upon it, as forcibly tending to establish his hypothesis. A little examination must satisfy us, that this is an argument which the facts connected with this point, and which are confessed to be true, will not authorize. It cannot be proved, that the presence of azote is essential in an article to make it nutritive, while we have ample evidence to demonstrate the reverse. From whence is the azote derived in herbivorous animals, that are sustained by articles in which this element is not to be found? In gum there is no azote, and yet, it frequently constitutes the only food upon which caravans are supported in toiling through regions covered with inhospitable sand. In maize, rice and sugar, it cannot be detected, and yet, they yield whole nations an abundant and healthful nutriment. These facts, together with those in the preceding paragraphs, which clearly proved the actual generation of elements in the blood, that did not pre-exist in the food, must demonstrate, that we are not warranted in ascribing the production of azote to any other than the same cause.

To this exposition of its source the chemist would object,

that it ascribes to the energies of life the ability of producing a simple substance, which, as he conceives, is violently assaulting an immoveably established principle. It would be a satisfactory reply to this objection, to say, that it is perfectly impossible for the chemist himself to tell with precision, what are simple and what are not simple substances. But we will not avail ourselves of what may be considered a subterfuge. The extent to which the animal economy generates simple substances, as they are chemically called, is not limited to azote alone. There are several others nearly, and some quite as much, entitled to be considered simple as azote. Thus it has been proved by the experiments of M. Bracconnot, that the earths, the alkalies, the metals, sulphur, phosphorus and carbon, which are regarded as substances of this kind, are developed by the energies of the vital principle.

Dr. Bostock, a most accomplished and able chemical physiologist, is constrained to admit, that some animals, the food of which does not essentially contain azote, discharge it in exceeding abundance in the form of urea. As the aliment of graminivorous animals is altogether insufficient to yield it thus abundantly, recourse is had by Dr. Bostock to the very equivocal supply furnished by pulmonary absorption. Independently of the manifest impropriety of deducing an inference from premises the truth of which is warmly contested, it is very certain that he has taken refuge in an argument the validity of which it is perfectly impossible to test. We can very easily determine the precise amount of azote discharged in the form of urea, but, I imagine it will be found impracticable to ascertain the quantity absorbed by the lungs for the protracted space of eight or ten days, the shortest time, evidently, which it would require to render an experiment of this kind satisfactory. Unless, therefore, this can be done, and a comparison drawn between the azote discharged and that absorbed by the lungs, to conclude with Dr. Bostock, that the latter is the source of the former, is to found an hypothesis upon a mere conjecture unsupported and indefensible.

Not willing that an hypothesis glaringly absurd and prepos-

terous should sink into deserved oblivion, M. Magendie, with his usual exhaustless experimental resources, determined, if possible, to give to it the substantial support commonly the result of that mode of investigation. His design was to prove, that in its ultimate analysis, the food contains all the elements, but particularly the azote of the animal body.

Dogs were fed by M. Magendie with articles entirely destitute of azote. For the first seven or eight, and even as late as fifteen days, they appeared to experience no inconvenience. Then emaciation immediately commenced, although the appetite did not seem to be impaired. Emaciation augmenting however, appetite was annihilated. About the twentieth day, ulceration of the transparent cornea commenced—rapidly progressing until it was perforated and the humours of the eye discharged. Dissolution took place between the 32nd and 36th days. There was displayed great corporeal extenuation; "there was no fat to be found; the muscles were reduced more than five sixths of their ordinary volume, the stomach and intestines were much diminished in size, and strongly contracted." The bile and urine had the characteristic attributes of those exhalations in herbivorous animals. The articles with which these animals were fed, were sugar, sweet oil, distilled water, and it may be proper to inform the reader, that M. Magendie satisfied himself that they "were reduced into a particular chyme, and that they afterwards furnished an abundant chyle."

Is it possible, that the mind of any physiologist, unenthralled by preconceived opinions, can be imposed upon by these experiments? The conclusion drawn from them by M. Magendie, is precipitate, unsound and unphilosophical. Is it not certain, that they did not die because their food was destitute of azote? If so, what is the reason that graminivorous animals flourish—their excretions abounding in azote, and yet their aliment unprovided with this principle? That the articles used were not altogether destitute of a nutritive power is clearly proved by the fact, that they appeared to experience no inconvenience, nor did they commence emaciating until the expiration of seven, eight and fifteen days, and can it be possible for an animal, pre-

viously healthy and hearty, to be in a state of absolute starvation for this length of time, and exhibit none of the evidences of gradual corporeal extenuation? Previously, moreover, the appetite was unimpaired; but now its vigour began to decline, and continued to do so until it was entirely annihilated; clearly corresponding in all its stages, with the progress of an intense gastric affection. Could any other result have been rationally anticipated? Is it possible, that an animal, buxom with health and living abundantly upon a rich animal diet, could be put upon that which is exclusively composed of meagre and impoverishing vegetable productions with impunity to his digestive organs? While, therefore, we have every reason to believe, that death, in the experiments performed by M. Magendie, arose exclusively from the unavoidable production of violent chylopoietic derangement, the evidence is yet to be adduced, to prove that the absence of azote in the article used, abstractly considered, had any agency in giving rise to the fatal result.

Moreover, it is a familiar fact, that every different species of animal, lives almost exclusively upon a diet composed of particular articles, for the digestion of which, their organs are particularly adapted. Does it, therefore, follow as a necessary consequence, that because certain articles are unfit for and destructive of certain animals they must be so of all? It is an established fact, that there are as many varieties of aliment abundantly supplied with the nutritive principle, but which are nevertheless injurious to certain animals, as there are varieties of gas richly impregnated with oxygen, that will not support respiration. Thoroughly did Hippocrates comprehend this subject when he said, "*alimentum unum et non unum.*"

If the view, however, which we have taken of the experiments of M. Magendie, does not appear satisfactory to the reader, let him consult those of Dr. Stark, and every doubt must be removed. This individual clearly proved, by a train of very conclusive experiments, that even those substances which afford the most nutrition, could not be used as the sole article of diet for any length of time without the stomach being seriously deranged.

Thus, from a sober and impartial investigation of the merits of the chemical hypothesis of exhalation, we are persuaded, the conclusion is irresistible, that it is altogether without foundation. We have fully proved, that there is really found in the exhalations, elementary principles which did not pre-exist in the food.*

Constituting nothing more than a property of the vessels of the absorbent system, it follows as a necessary consequence, that the functions that have been called *secretory* are under the exclusive control of the living principle, if we have succeeded in satisfactorily proving that absorption is emphatically a vital process. That this has been done the candid will not question. As there are those, however, who may not be inclined to unite with us in this conclusion, we shall institute a brief examination of the phenomena specifically belonging to exhalation, and we feel well assured, that we shall be able to demonstrate, that they all cooperate in condemning as utterly gratuitous and absurd every form in which the physical hypothesis has been presented.

As it presupposes the existence of all the exhalatory elements in the blood, and as it considers the capillary arteries as the immediate instruments of exhalation, it necessarily implies a coetaneous if not pre-existing alteration in the circulation, whenever there occurs either a normal or abnormal change in exhalation. It is unnecessary to advertise us, that the alteration in the former, in a majority of instances is nearly coetaneous with that in the latter, but it is not true, that they stand towards each other in the relation of cause and effect. There is no difficulty in adducing abundant instances of morbid exhalation, in which the integrity of the circulation is maintained unimpaired. Tumours are the result of morbid exhalation, and in the first stage, at least, they make no impression upon the action of the heart. Osseous depositions exist frequently,

* In refutation of these chemical vagaries much more might be said, and perhaps with advantage, but for the purposes of this essay, we hope sufficient has been already urged. To those, who desire a more ample and philosophic exposition of the subject, we take great pleasure in directing their attention to an exceedingly able paper to be found in the 2d. vol. of this Journal, entitled "*Thoughts on the Changes and their Causes, which are perpetually occurring in material Creation.* By Charles Caldwell M.D., Professor of the Institutes of Medicine, and Clinical Practice in Transylvania University."

for years without being suspected, and without affecting the vigour of the circulation. Collections of pus have often been detected after death, when no symptom during life denoted their existence. How frequently do we meet with dropsical accumulations without any corresponding change in the pulse? Numerous other instances could easily be adduced, but these are sufficient to show, that the integrity of the circulatory apparatus is not necessarily dependant upon a sound state of the exhalations.

If the capillary arteries were the immediate instruments of exhalation, or if the exhalations exist in the blood, extirpation of an organ could not prevent its exhalation from appearing in that fluid. But destroy the kidneys and urea cannot be detected in the blood; extirpate the testicles and the eunuch will be deprived of all the attributes that depend upon the semen.

In physics, there is no recognition of the power of vital elaboration, residing in the arterial absorbo-exhalents. Without it we shall be totally unable to explain the numerous morbid transmutations which are daily exhibited in the exhalations. Thus, the reception of a morbid impression by an organ will immediately produce an alteration in the state of the exhalation. In intermittent fever, during the chill, the urine is aqueous; during reaction, it is red; and in the sweating stage, a sediment is deposited. In diabetes it is unusually abundant: it is sometimes sweet and sometimes insipid. When inflamed the adipose tissue exhales pus instead of fat; the serous membranes sometimes exhale coagulable lymph, instead of serum. We often see the white unyielding bone, by a morbid nutrition, become soft, red and fleshy. In the same way a muscle in particular parts is transformed into bone and the cellular tissue into cartilage. The liver is sometimes nourished with a whitish fat, and then it degenerates into a cerebriform substance.

Nor are these the only illustrations of morbid exhalation. Look at the desolation of human happiness and of human life, produced by the virus in rabies canina, in syphilis, and in small-pox, all of which are morbid products. These are the unquestionable effects of a morbid exertion of the property of elabora-

tion—of that elaboration, in whose plastic hands, if we may use the expression, every exhalation is moulded into the particular form, whether normal or abnormal, under which it is exhibited. In the displays of its wonderful powers of metamorphosis, we see the creations of a purely vital agent, whose essence is to us we confess entirely incomprehensible.

Here we might properly close this particular subject of investigation, but we are induced to extend it still farther, in order to study a point of great interest, but, which, we are assured, has not been examined with sufficient attention. I allude to the form under which the elements of exhalation are removed from the blood. Here we are persuaded the generality of vital physiologists have fallen into the same error which they have so severely condemned, and successfully exposed in the chemical. By the latter it is contended, that all the elements of exhalation pre-exist in the blood, and by the former it is maintained, that the elective sensibility refuses admission to all the principles of the blood but such as are in relation with it, or rather, all those they consider unfit to be vitally transformed into a particular exhalation. Thus the fibrin of the muscles, and the phosphate of lime of the bones are of this character; and they are consequently removed from the torrent of the circulation by the arterial absorbo-exhalents of those tissues respectively. Now, I find it impossible to discover any difference between the chemical and the prevailing vital hypothesis but in reference to the particular manner in which the exhalatory elements are removed from the blood. The former denies and the latter defends the existence of the property denominated elective sensibility, while it appears to us very manifest, that they are both predicated upon a palpable error, i. e. the pre-existence of all the elements of the exhalations in the blood.

The vital hypothesis is extremely plausible to a certain point, but when we determine to pursue it beyond this, we at once find, that it is objectionable for the very reasons, which contribute so clearly to demonstrate, the falshood of the chemical theory. Thus, no speculative opinion can be more beautiful or appear more irresistibly fascinating, than when we see the elective sen-

sibility of the arterial absorbo-exhalents appropriating with most finished tact the fibrin for the exclusive use of the muscles; the albumen for the membranes; the phosphate of lime for the bones; the fatty matter of Vauquelin, &c. But when we attempt to account for a multiplicity of other elements, that are detected in the exhalations, but which cannot be found in the blood, its beauty is transformed into ugliness, and its fascination into repulsion. The invention of the physiologist, however, will not permit him to be thus easily foiled and defeated. He now assumes a new ground of defence, and as he cannot detect the proper materials in the blood, he boldly asserts, that the elective sensibility fabricates them.

We do not say, that this has been so plainly avowed, but we contend, that this is the legitimate inference. Is it not manifest, that as the elements of all the exhalations do not exist in the blood, and as some of them are supposed to be selected from that fluid, those, which cannot be detected there, must be derived from some other source? What other alternative is left then, but to suppose, that they are generated by the exertions of organic force. To the hypothesis thus plainly expressed is it not evident, that we may consider it justly obnoxious to the same arguments, that were employed to disprove the chemical theory.

The chemical identity of the blood, has been abundantly established by researches, the most diversified, concurrent, and unquestionable. In this respect, no experimenter of acknowledged standing has ever succeeded in detecting the smallest difference between arterial and venous blood. But does not this hypothesis rest upon a diametrically opposite supposition? It surely means to convey the preposterous idea, that the blood of one part is unlike that of another. Is it possible for a portion of a number of the principles of the blood to be removed and be deposited in the parenchymatous tissues and the identity of that fluid continue unimpaired? Certainly not; for it is of no consequence how inconsiderable may be the reduction of one principle, if all the rest do not participate to an equal extent in it, the identity of blood must be affected. This general participation is discoun-

tenanced by the fact, that all the exhalatory elements do not formally pre-exist in the blood; therefore, the loss must be sustained by those that do. Thus then, when in the exhalatory organs a part of the fibrin, gelatin, phosphate of lime, &c. have been removed, it is impossible for the constitution and nature of the blood to be precisely the same, as when it issued from the left side of the heart. But it may be said, that as the principles of the blood, which correspond with those that enter into the composition of some of the exhalations do not embrace the whole of which it is made up, is it not probable, that it is these, which serve to produce those exhalations, the elements of which cannot be detected in the blood. This it may be supposed would preserve its identity. This must unavoidably be the case if the hypothesis is true, for there is no other source from whence to derive those exhalatory elements, that are not discovered in the blood. In answer to which it is sufficient to say, that this is destroying the unity of exhalation, an attribute which we consider unalienable, by making it embrace two principles, that are essentially unlike, and which, I conceive, cannot possibly co-operate. Thus, some of the exhalatory elements exist in the blood, and elective sensibility has only to discover and remove them to their respective places of destination, while there are others, that have to be absolutely created out of the remaining principles of the blood. Can any reason be assigned, if this be true, why nature should have rendered this process so complex and unintelligible, while, on all other occasions, her operations are distinguished for astonishing simplicity? Or, can it be explained, if she designed the fibrin, gelatin &c. to correspond to those elements in the exhalations, why this principle was not extended to the rest?

This hypothesis is predicated upon another supposition equally destitute of a foundation in the established principles of truth. As certain of the elements of exhalation decidedly preponderate at particular periods of life, we would expect to find the same preponderance in the principles of the blood at corresponding ages. The blood of the fœtus, therefore, should be found unlike that of the adult; the blood of the adult should

differ from that of the toothless, bald, decrepit old man; the blood of the male from that of the female; the blood of man from that of the quadruped &c. But chemistry has failed in pointing out the difference. Every attempt, however pregnant with hope, has terminated in a complete abortion. Covered with confusion and dismay by the multiplicity of his failures, the chemist confounds arterial with venous blood; the blood of the plebeian with that of the patrician; the blood of man with that of the quadruped, and living with dead blood.

In certain diseases too there should be a manifest variation in the relative proportion of the principles of the blood. But in mollities ossium can the chemist discover less phosphate of lime in the blood than usual; when there is a general disposition to deposite bony matter in certain tissues, as sometimes happens, do we find this salt uncommonly abundant in the blood; when the cellular tissue is transformed into cartilage do we find the blood exuberantly supplied with gelatin; in elephantiasis do we find the usual quantity of fibrin reduced in the blood? A pointed negative must be given to every one of these questions. If then, in the preceding circumstances, the blood obstinately retains its healthy integrity, while there is every variety in the quantity of its elements in the exhalations, upon what principle of reason is it, that the specific elements of the former and those of the latter are considered as standing in the relation of immediate cause and effect?

From these considerations we are persuaded, it must appear obviously evident, that the hypothesis which maintains, that the elective sensibility of the arterial absorbo-exhalents selects the fibrin in its formal state from the blood to be deposited in muscles exclusively; the gelatin for the cartilages; the phosphate of lime for the bones, &c. is radically unsound and in its fundamental principle erroneous. The correspondence, therefore, which is observed to obtain between some of the elements of the blood, and some of those of the different exhalations, we are constrained to consider in the light of remarkable coincidences and not at all as specific cause and effect. They are destitute of the uniformity necessary to render them so.

Having now advanced thus far we are prepared, and it will appear to many a most paradoxical assertion, to say, that in elaborating the exhalations the whole mass of perfectly animalized blood is laid under contribution. This declaration we wish to be considered general in its application, as we know of no exhalation yielded by arterial blood, that should constitute an exception. The blood is an eminently vital fluid, and we conceive it to be just as easy to mould the whole of it into the form of a particular exhalation as a part.

Already is it confessed, that the arterial absorbo-exhalents do elaborate exhalations, which contain principles that did not pre-exist formally in the blood, and why may not this be the case with the whole of them? In a case that appears so plain, are we to be imposed upon by a mere coincidence, and deduce an hypothesis from a small number of facts when there are so many which assert our conclusion to be false? Is it not enough for us to know, that the elaborative attribute of the absorbent vessels does create principles, that did not exist in the blood? If in one instance she possesses this ability, in others exactly similar why is it not exerted? When the exhalatory elements cannot be detected in the blood, we gladly admit, that they are created by the elaborative energies of the absorbent vessels; but when the reverse of this obtains, this creative power is esteemed unnecessary. In the former case, who can say what principles of the blood are yielded up to the action of the arterial absorbo-exhalents; and as this is a difficulty which cannot be removed, is it not as probable, that fibrin, phosphate of lime, &c. can be as easily transformed into substances radically different from them as such, as any of the other ingredients of the blood? We are acquainted with no reason why they should claim an exemption from this process. Many of them owe their existence in the blood, in the state in which they are found, to the influence of it, and why may not the same power be capable of exhibiting them in the exhalents under a totally different form?

These transformations are constantly going forward wherever the influence of the living principle is felt. They commence in the plant, and progress step by step until they become

fully and eminently impregnated with life. In the plant, the first step of the process of elaboration unquestionably begins. Substances are extracted from the soil, that are evidently unfit in their formal state to be elaborated by the powers of the animal economy, but from them are created elements which did not exist in the earth, air or water employed by the plant, and which are to animals and man abundantly nutritive. Subjected to the action of the organs of the animal economy in passing through the second stage of the process of elaboration, chyme is produced, and in it we find principles that did not pre-exist in the plant. In the third stage, during which chyle appears, elements are developed, that did not exist in the chyme and the quantity of those that did, become, more abundant. In the fourth stage, which consists in the transformation of chyle into lymph, the principles that existed in the former are found to be in greater abundance in the latter. In the fifth stage, in which lymph is converted into blood, the elements augment in quantity. In the sixth stage, which consists in the elaboration of blood into numerous exhalations, new principles appear and several of the old ones become more abundant.

Thus, in the conversion of chyme into chyle the whole mass is elaborated and not a part; in the conversion of chyle into lymph the whole mass is elaborated and not a part; in the conversion of lymph into blood the whole mass is elaborated and not a part; and in the conversion of blood into the different exhalations, why should a part and not the whole be elaborated?

When we treat of the functions of the venous absorbo-exhalents, it is our design to attempt to prove, that the debris of the organs are thrown directly into the veins and converted into venous blood. If it is, therefore, practicable to convert the fibrin of the muscles; the gelatin of the cartilages; the albumen of the membranes; the phosphate of lime of the bones, &c. into venous blood, why should the idea appear more absurd or the difficulty seem greater, to transform the perfectly animalized arterial blood *en masse* into fibrin, gelatin, albumen, phosphate of lime, &c.? This argument is strengthened by the

fact, that the chemical elements of arterial and venous blood, as far as can be ascertained, are precisely identical.

In conclusion let us remark, that the reader may labour under no misapprehension, that the hypothesis above suggested does not conflict, in the least, with the legitimate influence of the presiding energies of elective sensibility. Thus, the perfectly animalized blood is always in relation with the elective sensibility of the radicules of the arterial absorbo-exhalents, while the chyle, which is floating with it, and which has to be subjected to the elaborative energies of the lymphatic absorbo-exhalents, never is, except in disease. With regard to the latter order of absorbents, the reverse of this precisely obtains. While they absorb the chyle they refuse admission to the perfectly animalized arterial blood.

It should have been remarked above in support of our hypothesis, that when the elaborative power of the arterial absorbo-exhalents is so decidedly impaired, as to prevent them from accomplishing upon the blood the necessary changes it is then exhaled, in its formal state, instead of the proper exhalation. Thus, after the injudicious suppression of hæmorrhage the breasts have frequently exhaled pure blood instead of milk. In the French Journals, numerous cases may be found recorded of the exhalation of a rose-coloured milk after accouchment. In the mal de Siam, most of the organs exhaled blood. This is a frequent occurrence with the kidneys in small pox. Instances of the exhalation of blood may be found in the writings of Zacutus Lusitanus, Hilden, Merat, &c. but particularly in those of the latter and in great abundance.

[TO BE CONTINUED.]

ART. III.—*An Essay on the Origin of the Plague.* By JOHN ESTEN COOKE M. D. &c.

THE origin of the disease called the plague, which frequently ravages the countries around the borders of the Mediterranean sea, is a subject which has for ages attracted the attention of physicians. The prevalent opinion has been that a specific contagion is the cause. But even the advocates of this doctrine have admitted that a peculiar state of the air is necessary to its production. Thus the celebrated Dr. Mead, though he relates ridiculous stories in proof of the powerful contagion of the plague, in plain terms allows that a certain state of the atmosphere is necessary to the prevalence of this disease.

A specimen of the stories alluded to is contained in the following passage from his discourse on the plague. "A very remarkable story lately communicated to me by a person of undoubted credit, is too much to the purpose to be here omitted. The fact is this. In the year 1726, an English ship took in goods at Grand Cairo, in the time of the plague's raging there, and carried them to Alexandria. Upon opening one of the bales of wool in a field, two Turks employed in the work were immediately killed; and some birds which happened to fly over the place, dropped down dead."*

But notwithstanding this and many similar absurd stories in his book, which are totally opposed to the well known fact, that as soon as the plague ceases in Egypt, in mid-summer, the clothes of the dead are sold publicly without the least apprehension of evil; Dr. Mead plainly shows by many well attested facts, that a certain state of the air is necessary to the continuance of the plague, and even assigns as one cause of the spreading of the disease, "*a corrupted state of the air.*"†

Dr Russel also, a later and a more judicious writer, although he believed this disease to be contagious, repeatedly asserts the

* Mead's Discourse on the Plague, p. 89. † Ibid p. 60.

necessity of a particular state of the air to its prevalence. He says, "The state of the air instrumental in propagating the plague varies in its degree of power, as well as in its duration."* He observes, "Should ever that state of the air, *without the concurrence of which the contagion of the pestilence never spreads, or ceases to act*, be discovered and ascertained by unequivocal marks, the dread of the plague universally prevalent, would be greatly diminished; more effectual means of preservation would be found out, and the application of them might safely be limited to certain seasons. Experience in Turkey, where generally no precautions are taken in the times of pestilence, clearly evinces, that in a certain state of the air, a communication with infected places may subsist without any material consequence."† "I consider it therefore, as an established fact in the Levant, that commerce and intercourse with infected towns, is sometimes attended with no bad consequence. The same thing may perhaps be asserted, without restriction of all countries; but till the signs indicating *a pestilential constitution* be ascertained, no particular year can be declared exempt from danger."‡ Again he says, "We have already seen that the plague at Marseilles began to decline long before it was in the power of the police to attempt the general expurgation of the city, which took place in the sequel: and that the measures taken for that purpose, however vigorously pursued, did not succeed in extinguishing the distemper entirely, which continued lingering several months after. Had therefore the pestilential constitution of the air continued, there can be little doubt that the plague would have spread at Marseilles, to a certain degree, in the year 1721, in the same season which in the year preceding had proved so fatal. We have seen also at Aleppo in 1762, that the plague ceased completely without any exertion whatever on the part of the police; and experience in other places of Turkey, as well as at London, Nimeguen and Dantzic, furnishes similar examples.

"Hence it may be inferred, that the termination of the plague

* Russel on the Plague, p. 273. † Ibid, p. 280. ‡ Ibid, p. 281.

of Marseilles was owing principally to a change in the constitution of the air."*

These quotations from avowed contagionists show clearly that a peculiar state of the air is necessary to the production and prevalence of this disease. It is the object of the present essay to show that this state of the air is the same with that which produces the autumnal epidemics of temperate climates: and in order to this it will be shown, that the plague arises in the same circumstances with them, prevails more or less in proportion to the prevalence of the same circumstances which favour them, and ceases in the same circumstances with them.

The circumstances which give rise to autumnal epidemics, have been shown, in a preceding paper in this journal, to be heat and moisture operating upon vegetable matter; and therefore that marshy situations in hot weather, or upland countries in wet and hot summers, are the seat of these epidemics; that where moist vegetable matter abounds, the disease is in proportion to the heat of the weather; that extreme drought by drying up a marshy soil will, even in hot weather, put an end to it; and that cold weather will do the same. These are equally true of the plague.

First, with regard to the state of the places in which this disease has from time to time prevailed.

England was very often ravaged by the plague in former times. That country is flat, moist, and in many parts marshy. The fens or marshes of Essex and Lincolnshire are often mentioned in the medical writings of that country. London, which was so often harassed and once almost half depopulated by the plague, is situated in a low place originally marshy, with marshy rivulets running through it. Westminster was formerly separated from the city by a morass. A well known part of the city is called the Strand, having evidently taken its name from its formerly having been the edge of the water. The borders of the river Thames were generally marshy. Many other towns of England were formerly ravaged by the same disease, and they

* Russel on the Plague, pp. 282, 283.

also are situated in low places, and almost universally had marshy ground near them.

Holland was equally harassed, perhaps more so than England. That country was one vast marsh. The inland towns as well as the sea-ports were surrounded with marshes and were likewise ravaged by the plague.

Many towns of Germany also, formerly ravaged by this disease, are situated on rivers and near marshes.

In Italy the plague raged with the utmost violence in the towns situated in the valley, through which run the Po and its branches. These rivers are very apt to rise to a great height, and formerly overflowed their banks upon every occurrence of this kind. The towns on the other side of Italy, in the champaign country on the borders of the Mediterranean sea, through which run many sluggish streams, and where marshy ground was abundant, were likewise harassed in the same manner.

These countries are not at this day ravaged by the plague, but yet are subject to epidemics which prevail at the same periods of the year in which the former disease made its appearance. They are just as much exposed as they ever were to the danger of receiving it from the other side of the Mediterranean, by means of commerical intercourse. The cause of the change is therefore to be looked for in the change of the constitution of the air of those European countries: and when we inquire into this matter we find that a prodigious alteration has taken place in them.

Formerly England was a wet marshy country in the extreme: now it is drained and cultivated with the greatest care, and is comparatively dry. Formerly London was an actual marsh; and the shores of the Thames were mire. But now, for miles of its course through that city, the banks of the river are wharfed. Formerly that city was, with few exceptions, a crowd of huts, without regularity, situated on crooked and narrow streets, and abounding with filth. Most houses had no floors and the earth was covered with rushes cut from the neighbouring marshes. These habitations were destroyed by the great fire in 1666, immediately after which the city was laid off in a far

better manner. Since that time, since the river has been banked out, since the marshes in the city have been filled, and the marshes around the city have been drained, and for ten miles around, the country cultivated like a garden, and the streets have been made wide and ventilation has been free, the plague has scarcely appeared. That this explanation of the cause of its absence is correct, is evident from the fact that the same malignant fevers which always preceded the plague; that is to say the minor degrees of that epidemic; have appeared since those times, but have never arisen to the same height, those malignant fevers gradually moderating as that city improved in the particulars abovementioned; and at the present day, when the city has become dry and cl an, and the country for many miles around is highly cultivated for the supply of the city with vegetables, these diseases are little known except in the confined, obscure and filthy spots, which in some degree still resemble what the whole formerly was. Pringle informs us that in his time, that is in the middle of the last century, "in some of the lowest, moistest, and closest parts of the town, and amongst the poorer people, spotted fevers and dysenteries were still to be seen, but were seldom to be heard of amongst those of better rank, living in more airy situations."

The same may be said of every other part of Europe that has been mentioned. Holland is infinitely drier than it formerly was. The same is true of Italy. The country on the borders of the Po, in particular, is a most beautiful and highly cultivated one—preserved from inundations by numerous embankments.

The evidence adduced is abundantly sufficient to show that in Europe the plague arose in situations in which miasmata must have abounded in hot summers; and it appears from the accounts which we have received from the writers of that day, of the number of persons who died in the several months, that this disease increased with the increasing heat of summer, was at its greatest height when the temperature was highest, and declined as it declined.

This is strikingly evinced by the following table. The figures

1593 &c. in the first row indicate the years: the figures in a line with the names of the months indicate the number of persons who died in the respective months in each year in London; excepting the last column of figures, which contains the numbers that died in Dantzic in 1709.

	1593	1603	1625	1636	1665	1709
April	138	26	85	37	2	
May	167	83	224	162	43	
June	468	362	954	440	1060	319
July	2930	2999	5887	456	5667	1313
August	2880	8919	16455	1239	17036	6139
September	2200	12504	9979	3856	31569	8303
October	1260	4012	1514	2786	9444	4932
November	710	1352	256	2602	3449	1961
December	290	324	37	640	734	584

It is evident from this statement that the mortality kept pace with the increasing heat of the season, declined with it and ceased in cold weather. It is even observable that the very same kind of variations that are observed in the epidemics of hot weather in this country, occurred in these epidemics in London and Dantzic. Thus, in the year 1793 in Philadelphia, the fever was on the increase in September, and was at its height as late as the second week of October; and we know that the weather was in that year excessively hot in October: but in 1798 the fever was suddenly checked, when raging with uncommon violence, in every part of the city as early as the 29th of September by a frost. So in the table above, the disease was at its height in London in September in three years of the five stated: but in 1593, and more particularly in 1625, the mortality of September was less than that of August. There is another circumstance deserving especial notice. The summer of 1798, was excessively dry and hot; and the fever of that year was the most violent that ever raged in Philadelphia. In just such a season, in 1665, the severest plague ravaged London that that city had ever experienced. The summer of that year was so hot and dry, that cattle were killed for want of grass to feed them.

From the statements now made, it is evident that in Europe the plague prevailed in places in which miasmata must have abounded in hot weather; and that the heat of summer was the time in which it prevailed, and with greater or less force in proportion to the heat of the weather; and consequently in proportion to the abundance of miasmata.

If we examine into the state of the countries of Asia and Africa at the times when the disease prevails in them, we shall find the same truth abundantly supported.

Egypt is often ravaged by the plague. The lower part of that valley is a very rich and level country. It is intersected by the outlets of the river Nile, some of which have, in consequence of changes in the course of the waters, become stagnant; and it has lakes and numerous canals. The whole country, after the waters of the annual inundation retire, is left in a marshy state; and the lowest places continue marshy until midsummer. Such places are to be found near the towns and in them.

The state of the country is therefore remarkably adapted to the production of miasmata in hot weather, as long as the moisture continues;—and this is the very time of the prevalence of the plague in that country.

The Nile begins to increase in June, but it is some time before it swells beyond its banks. Assalini says that on his arrival in that country, he encamped on a dry barren and scorched soil on the 5th of July, marched for Cairo on the 7th along the banks of the Nile, and arrived there on the 23d. On the 19th of August the dikes were cut to allow the water to enter into the squares and gardens of Cairo. The country is covered in September and October, and is quite wet until the last of October, and sometimes later. By this time winter is approaching fast; but as even in winter the temperature of that country is high, fever and dysentery make their appearance. In the month of February the heat, we are informed, is scarcely supportable by an European,* and as the season advances, the temperature rapidly increases. The plague appears in the spring,

* Rees's Cyclopedia; Art. Egypt.

increases with the increasing heat of the weather; but the same cause drying up the country rapidly in June, the disease declines and terminates about the last of that month or the first of July, when the very marshes and lakes have been dried up, before the temperature rises to its greatest height, which is in August.

It may be observed that there is a remarkable difference between the circumstances in which the plague ceases in Egypt and in the European countries which have been mentioned. In the latter the disease continued until checked by cold weather; in the former, it ceased before the heat had arrived at its greatest height. But this difference corroborates the doctrine maintained. In the places mentioned in Europe there was always moisture enough; and the disease appeared only when the heat became excessive; and it continued as long as the temperature continued high enough. The disease therefore ceased for want of a temperature high enough to sustain it. In Egypt, on the other hand, it was moisture that became deficient. The disease appeared as soon as the temperature was sufficient to produce it, and continued to increase with it: but, no rain falling in that country, and the moisture left by the inundation gradually evaporating, by the last of June or first of July the whole country becomes perfectly dry: even the marshes and shallow lakes are dried up.* The disease, therefore, ceases when the moisture fails; which is precisely what ought to occur, if the disease depend on the concurrence of heat and moisture. This is what occurs in temperate climates in diseases admitted to depend on the concurrent action of heat and moisture on vegetable matter. When a country is perfectly dried up, the ordinary autumnal fevers cease.

It should be noticed that some have attributed the cessation of the plague in Egypt about the last of June, to the waters of the Nile then covering the country. This however is certainly incorrect. The Nile indeed begins to rise in June; but the river at that time of excessive dryness is very low, and the

* Assalini on the Plague, p. 4.

increase is gradual; so that some time elapses before it fills the banks, and it gradually spreads over the country. Assalini says that in July the French army marched along the banks of the Nile and arrived at Cairo on the 23rd; and that on the 19th of August the dikes were cut to let the water into the gardens of that city.*

The same author states that the French army at Jaffa in Syria, in 1799, encamped near some marshes, and soon after the plague broke out among them.†

Dr. Cowdery, an American surgeon, for some time a prisoner at Tripoli, gave to Dr. Mitchell of New York the following information respecting the prevalence of the plague in that city.

That city is situated on a sandy and barren desert. It was remarkably healthy also; but he was informed that it had formerly been infected with the plague. Being desirous to discover the cause of the change, he made inquiries among the old officers of government; and ascertained the following facts. At the time in which the plague prevailed, there was a large ditch at the foot of the southern and eastern ramparts, which had been dug for the defence of the city. This ditch contained water, and during the hot weather the plague broke out; and the Bedouin Arabs, who were encamped on the borders of the ditch, were the first sufferers and perished almost entirely. Since that time, by the operation of the rains in winter and the winds in summer, the ditch had been filled with sand; and the city had become healthy.‡

We have a very minute account of the circumstances in which the plague prevailed in Aleppo in Syria in 1760, 1761 and 1762, in the work of Dr. Russel; and the whole sustains the doctrine here advocated in a remarkable manner.

The city of Aleppo, which contained at the time Dr. Russel wrote about 250,000 inhabitants, is situated upon several small hills and in the intervening valleys. There was at the time he wrote a broad ditch around the town, partly filled up. A stream of water runs through the city, and sometimes overflows its

* Assalini, p. 6. † Ibid, p. 7,8. ‡ Med. Repos. vol. 9. pp. 155 &c.

banks. The population, in many parts, was excessively crowded in filthy habitations, situated in low damp places, and many of them under ground.

The climate is very hot and dry. The herbage is generally burnt up by the end of May; and in the months of June, July and August, there is often not more than one day's rain.

In this state of things, as was to be expected, there is an annual epidemic, in which the usual intermittents, remittents, dysenteries, &c. are observed. Occasionally also, that more terrible epidemic, the plague, prevails; and it shall now be shown from the facts stated by Dr. Russel, that this occurs precisely at those times and in those circumstances in which the cause of the ordinary epidemics must have been more powerful, and the epidemic itself, consequently, more severe; and that this dreaded disease is regulated in its rise and its termination, its increase and its decline, by the same circumstances which govern other hot weather epidemics.

The plague prevailed in the years 1760, 1761 and 1762, and did not in the year preceding and that following those mentioned. Dr. Russel has enabled us by his minute account and his tables of the weather in these years, to make the following statement relative to the temperature and the quantity of moisture in the several years in question.

With regard to temperature, there was remarkably little difference between the healthy and the sickly summers. Thus the highest degree of the thermometer observed in the month of March in the four years from 1759 to 1762, was twice 60° and twice 59°. In April it varied more; but in May in three years, it was 80°, 82°, 82°. In June it was 94° in one year and 89° in the next four years; and in July it was twice 95° and three times 94°. This is so remarkable an uniformity of temperature, that no extraordinary difference in the epidemics could have arisen out of the difference between them in this respect.

With regard however to the moisture of the different years, there was a marked difference. The years in which the plague prevailed were much the wettest, and, moreover, the excess

of rain in the sickly years, was particularly great in the hot months of May and June, when the thermometer was at 80°, and the disease most prevalent. The result is obvious: heat and moisture did not occur together in the healthy years 1759, and 1763, in near so great a degree as they did in 1760, 1761, and 1762, in which the plague prevailed.

To enter a little into particulars, in the first six months of the five years, viz. the three years in which the plague prevailed, and the two years which immediately preceded and followed them, the number of days of rain were as follows. Some few days of snow are included as days of rain.

In the first six months		In May and June	
of 1759	32 days rain	of 1759	1 day's rain
of 1760	69 do.	of 1760	11 do.
of 1761	52 do.	of 1761	6 do.
of 1762	54 do.	of 1762	10 do.
of 1763	42 do.	of 1763	3 do.

Nothing can be more striking than this statement. There was one day of rain in the year 1759 in the hot months of May and June; and although it was much hotter, in those months, in that year than in the four following years, the city was healthy. In the year 1763, there were but three days of rain in May and June, and the city was healthy. But in 1760, 1761 and 1762, there was in May and June an average of nine days of rain, and it was very sickly.

The same thing is observable on comparing the years 1743 and 1744, in which the plague also prevailed in Aleppo, with the five years abovementioned. The average of the days of rain in the two warm months, May and June of the plague years, 1743 and 1744, was *eight and a half days*; of the healthy years, 1759 and 1763, *two days*; and of the plague years, 1760, 1761, and 1762, it was *nine days*. The reason of selecting these two months is, that the temperature of the season did not before May arise to that height which is known to produce epidemics of the highest grade. The highest degree to which the mercury in the thermometer arose in April 1760 was 68°; in 1761,

78°; in 1762, 66°; while in May it arose, in 1760, to 80° in 1761, to 82°; and in 1762, to 82°; and in June to 89° in all three of those years.

In comparing the extent of the disease in the different years 1760, 1761 and 1762, the same principle evidently governed. Thus, in 1760 there was more rain than in any of the years mentioned; but an unusual share of it fell in March, in which month it rained eighteen days; and in March and April together, it rained thirty-seven days. The consequence of this was that the spring of this year was extraordinarily wet and cool, and it continued cooler than common through June and July. Although therefore the unusual quantity of rain brought unusual sickness in that warm climate, the disease was not as severe in 1760 as in 1761, on account of its being cooler in the former.

In the year 1761 the plague was not so severe as in 1762; and on turning to the table of the weather, the reason is obvious. In 1761 more rain fell in the forepart of the spring than in 1762. In 1761 it rained in March and April *twenty-three days*; in 1762, only *sixteen;* while in the hot months, May and June, the thing was reversed, and more rain fell in 1762 than in 1761. In 1761 it rained in May and June, *six days;* while in 1762 in the same months it rained *ten.* It is evident therefore that heat and moisture concurred in greatest degree in those years in which the disease was most prevalent. The same is shown in the clearest manner by a still closer examination.

It has been stated that in 1761 it rained *six days* in May and June, and *ten* in the same months of 1762; and that the disease was more severe in 1762, in correspondence with this greater quantity of rain falling *in the hot weather.* It may be remarked further, that the tables of Dr. Russel show that the greater part of this excess of rain in May and June 1762, fell in the latter month when the heat was still greater, the mercury in the thermometer sometimes standing at 89°, and the temperature still increasing; and we also find that the disease increased immediately after this greater fall of rain. This will appear in a striking light on inspecting the following table.

In 1761, in the week ending		In 1762, in the week ending	
June 8	670 died	June 7	710 died
		A shower this week	
15	543	14	874
Some showers fell in the 2nd week		Great rains in the 2nd week	
22	505	21	1208
29	612	28	1273
July 6	708	July 5	1472
13	483	12	998
20	296	19	506
27	236	26	379
Aug. 3	102	Aug. 2	232
10	110	9	136
17	90	16	112
24	108	23	71
31	66	30	68

The effect of the fall of rain is evident. After the 8th June 1761, the disease was declining. Some showers fell in the second week, and there was a manifest increase in the number of deaths, for a fortnight. But the quantity of rain being very moderate, the disease soon began again to decline. Mark however, the difference in 1762. A shower in the first week increased the number of deaths; and the heavy rain in the second, produced a corresponding effect. In a few weeks, however, in this year also, the returning drought of that arid climate parched up the country, and the disease declined as in 1761; so that by the end of August the deaths had fallen to the same number in both years.

It appears from the foregoing statements that the plague, in every part of the world which has been ravaged by it, prevails precisely in those circumstances in which miasmata must abound; and expires or ceases exactly when they cease to abound; and, like other miasmatic diseases, the plague ceases whenever the concurrence of heat and moisture in action on vegetable matter ceases, whether the cessation arise from the disappearance of moisture or from the cessation of high temperature. Thus, in Europe, in the marshy situations in which it prevailed, moisture always abounding, the disease never prevailed except when the temperature of the season was very high; it increased as that increased; and ceased when that ceased. In Aleppo, situ-

ated in an arid country, where vegetation is commonly burnt up by the last of May, and where it rarely rains throughout June, July, and August, the disease only appears when the season is unusually wet; it continues as long as moisture continues; increases with an occasional fall of rain, the increase being in due proportion to the rain that falls; and the disease ceases when the moisture is totally exhausted, even before the heat of the season has arrived at its greatest height. In Egypt, a country in which, except along the sea-coast, no rain falls, the same principle is as strikingly exemplified as in either of the others; the moisture necessary to the production of the disease being supplied by the inundation of the country by the river Nile. This moisture with the concurrence of heat, as soon as the season becomes hot enough in the spring, produces the plague. The disease increases with the increasing heat of the season; but ceases in mid-summer, before the temperature has attained its greatest height, at the time when the very lakes have become dry, and the whole surface of the earth is parched and hardened like a sun-dried brick.

It is evident therefore that this disease arises in the same circumstances of situation and of heat and moisture which give rise to the severe epidemics of our own country; increases in the same kind of weather in which they increase; and ceases in the same circumstances in which they cease.

The analogy between the plague and our severe epidemics, is observed also in the symptoms. The plague according to Dr. Russel, commences often with a slight shivering or sense of cold; this is soon followed by a fever more or less violent; the fever is a manifest remittent of the tertian form; in the exacerbation the head is more or less affected with stupor or delirium, which in greater or less degree wears off in the remission; the disease sometimes commences with convulsion, as in our fevers; there is occasionally a diarrhœa, but the patient in general is costive; purges were not given during the plague of 1760 &c. the people having a great prejudice against the most gentle medicine of that class; and therefore the state of the biliary secretion could not be ascertained, except when discharges from the

bowels occurred without the use of them; on these occasions they were sometimes black; nausea is a common symptom, as well as vomiting; the patient often throws up bile; frequently nothing is cast up from the stomach, except the liquids which the patient may have recently swallowed; this vomiting often continues until death; the patient complains very much, likewise, of soreness in the epigastrium, and of a burning sensation in that part, and could not bear to have it touched: a vomiting of black matter often occurs about the time of death.

The analogy between this disease and our severe autumnal epidemics is evident from these symptoms; but there are some not yet mentioned, which have been generally considered as peculiar to the plague. Buboes, parotid swellings, carbuncles, and those red spots on the skin called petechiæ and vibices, are alluded to.

These symptoms however are far from being peculiar to the epidemics of Africa and Asia. They frequently occur in this country, and are mentioned by several authors. Dr. Potter in speaking of the yellow fever of Baltimore of 1798, says that many of them were accompanied by inguinal buboes.* Dr. Hildreth in his account of the epidemic fever of Ohio in 1822, informs us that it was "attended with symptoms of malignity, such as petechiæ and glandular swellings, most commonly of the parotid glands.† Dr. M'Cabe of Leesburg in Virginia, informed me that in a case of autumnal fever of very high grade under his care, a carbuncle appeared on the chin. I have myself, in several epidemics, observed buboes, parotid swellings, and carbuncles. Petechiæ and vibices are common in epidemics of high grade, and particularly when, as in the plague of 1760 &c., cathartics are not used.

Moreover, these symptoms are not only not peculiar to the severe epidemics of the countries on the southern shore of the Mediterranean; but in many of the worst cases of those epidemics, they do not appear. Dr. Russel informs us that in the worst cases they were not to be found. He divides the cases of the plague into six classes. In the first, in which the patient

* Medical Recorder, No. 4, p. 561. † Phil. Journal &c. No. 17, p. 108.

died in one, two, or three days, buboes or carbuncles seldom appeared: in the second class, very few were to be seen: in the third, they frequently did not appear: in the other classes they were common.

In those cases in which they did appear, they did not influence either the course of the disease, or the result. A profuse sweat on the fifth or seventh day placed the patient out of all danger, and the fever disappeared long before the buboes came to maturation: and even if those tumours disappeared, there was very little difference observed in the course of the fever, or the quickness of the recovery, if the patient had sweated. If this took place he recovered quickly; if not, slowly. It is evident, therefore, that the buboes, &c. had no influence over the progress or termination of the case, *the essential point being a profuse sweat on the fifth or seventh day*, which terminated the disease, while the buboes, &c. were still advancing.

These buboes, &c. moreover, were of no consequence when there was no fever; as was the case in numerous instances. In such cases they were often concealed, and little attention was often paid to them when their existence was known to the rest of the family.

Setting aside these symptoms, which are neither essential nor peculiar to the plague, this disease is strikingly like our severe epidemics: So perfectly alike, indeed, that I have read Dr. Russel's account of the course of the symptoms of the plague to a number of physicians, every one of whom considered it as a description of our severe epidemics. On being requested to say what disease was described in the account read to them, one said it was the yellow fever; another, that it was our country epidemic of the highest grade; and all gave similar answers.

There is also another strong consideration. At the same time that these cases appear, which are described as the plague, intermittents, dysenteries, &c. are common. Now, these prove the presence, at the time, of the same cause which produces our severe autumnal epidemics; and that cause, in such circumstances as those in which the plague prevailed, (viz. uncommon rains in the summer, when the temperature is at 80° and 90°

and even 94°,) always produces cases precisely similar to those described by Dr. Russel as cases of the highest grade or first class of the plague; in which, he says, no buboes &c. appeared. It is manifest, therefore, that there was then present a cause, capable of producing just such cases as he describes as cases of the plague; and which, from all our experience, must have produced them. Consequently the plague cases were not to be distinguished from those which were produced by miasmata.

ART. IV.—*Case of Phlegmasia Dolens successfully treated by the application of the Bandage.* Communicated in a letter from DR. WILLIAM T. YOUNG, of Triana, Alabama, to PROFESSOR DUDLEY.

SIR—Believing that you do always feel a lively interest in the success of the bandage over morbid action, in whatever part of the system it may take up its abode, I had intended at this time, to give you, the full history of a case of Phlegmasia Dolens, of long standing; but am prevented from doing so, in consequence of misplacing my notes made on the case: hence I can only communicate the termination of it for the present.

The subject of this truly painful disease was, at the time of her illness, in the 36th year of her age, and had given birth, a few days before, to a fifth child. She has light blue eyes, and dark coloured hair; sallow complexion; sanguineo-bilious temperament; fat habit of body, and rather lower than the ordinary stature of her sex. When I was requested to visit her, the first time, with the attending physician, her situation presented to my mind a truly lamentable aspect: for she had been confined to the bed upwards of three months, and had more the appearance of a dead body dried, than any thing I could imagine, with the exception of her lower extremities, which were

swelled to that degree, that the skin covering them appeared just ready to burst in several places.

As every rule of practice that could be made to bear on the case was gone through with, by her able physician, before I was called in, with the exception of the bandage, I really was at a loss, to know what farther to advise for her then situation; but after examining the case attentively again, I came to the conclusion to recommend, in opposition to their prejudices, the judicious application of the bandage, not however from my own knowledge of its efficacy, for I had not yet used it in this disease; but from your testimony of its utility in that affection; and although my advice was reluctantly received by the physician, as well as her friends, I, in their presence, applied it, on both her legs, from the toes to her hips. You cannot imagine my gratification on my next visit, five days after, at finding the bandage quite loose, the limbs much reduced in size, and the patient decidedly better. At this visit I took off the bandage and re-applied it firmly and neatly on the limbs; and by the use of it, with a generous diet, the lady was able in three weeks, from the date of the first application, to walk across her chamber, and in two weeks more, was restored to her usual good health. The success of the bandage in this case, may go to prove, to every candid enquirer after truth, that the true pathology of that disease, has been very imperfectly understood.

ART. V.—*Case of Gun-shot wound of the elbow-joint, treated by the Bandage.* By DR. JACOB S. SWAN, of Henderson County, Kentucky.

ON the evening of the 9th July last, I was requested to visit J. C. son of P. C., who on that day had been engaged with a party in hunting deer, and in some way, not easily accounted

for, received from one of the huntsmen a large buckshot in the elbow-joint, the ball entering at the inferior edge of the tendon of the biceps muscle, and coming out a little behind the radius. The hæmorrhage had been very alarming, but had been restrained by some person's having bound a silk handkerchief firmly around the joint. I enjoined quietness, and a rigid antiphlogistic regimen, and without any other means, things went on very well for forty-eight hours; when pain and swelling supervened to a great degree, and I began to apprehend that the case would terminate in the loss of the boy's limb. At this juncture of matters I was resolved to try the bandage as recommended by Professor Dudley, and accordingly prepared one, and having soaked it well in strong whiskey, I applied it from the fingers to the shoulder. The pain was in a short time entirely relieved by this application; and upon visiting him the next day, I found it necessary to reapply the bandage; the swelling having so much subsided as to render it quite loose. The bandage was thus continued for ten days, being occasionally removed and re-applied as circumstances called for, when the arm was well; no deformity remaining except the scars caused by the entrance and exit of the ball; the joint, which was a little stiff for several weeks, has now, (20th Sept.) entirely recovered its functions. I consider the joint not fractured in this case, but great injury was done it; and my opinion is, that the brachial artery was completely divided. I think that the case would certainly have terminated unfortunately, treated in the usual way. That the application of the bandage in wounds and accidents, as well as other cases, in which it is used and recommended by Professor Dudley, is original with that gentleman, is beyond all possibility of cavil; and reflects the highest honor upon his talents and skill as a surgeon.

MISCELLANEOUS INTELLIGENCE.

Observations on the Blood. By William Steevens, M. D.—At a late meeting of the College of Physicians, a paper on the above subject was read by Dr. Steevens, who has, for many years, been a practising physician in the West Indies. As the views which Dr. S. entertains are novel, and the means which he proposes for combatting a dreadful scourge of the human race, are simple and practicable, we have endeavoured to collect the substance of the writer's observations as accurately as possible, to lay before our readers.

Dr. S. sets out by remarking that a malignant form of yellow fever is sometimes seen in the West Indies, which, from beginning to end, evinces, by the symptoms, little or no affection of the solids, and leaving, after death, no cognizable trace of disease in the brain, stomach, intestines, or other viscus, whose derangements are supposed to be the cause of fever. In those fatal cases, there is no excitement, in the commencement, adequate to the injury of organs, and we can only ascertain the real cause of death when we come to open the heart, and find in its cavities, a dissolved fluid almost as thin as water, and black as ink—a fluid evidently incapable of stimulating the central organ of circulation or of supporting life. The fluid is equally black and thin throughout the vascular system, all distinction between arterial and venous blood being obliterated.

An attentive observation of some cases of fever first led the author to believe that, in this disease, the influence of the nervous system is less operative than is generally supposed, and that the blood is infinitely more concerned than the solids, in an etiological point of view. He was thus led on, not only to attentive observation, but to experiments on the state of the blood in fever, some of which he has now submitted to the profession. The writer remarks that it has been fashionable, for more than a century, to overlook the pathology of the fluids; yet to the practical physician, the morbid changes which the blood undergoes is valuable beyond all calculation.

On examining, after death, the black and dissolved blood that has been taken from the hearts of those who have died of the yellow fever, it was evident that great changes had taken place. *First*, the blood was more fluid, partly from an excess of serum, probably produced by

a stoppage of all the secretions—partly, perhaps, from the non-formation of fibrine in fever, or its more rapid exhaustion than in health. In the first stage of the disease, the structure of the red globules is frequently deranged, as is evident from the colouring matter being often detached from the globules, and dissolved in the serum, giving to that part of the blood, when it separates from the fibrine, a bright scarlet colour. As the disease advances, this red colour is lost, and the whole circulating mass becomes black and thin. *Secondly*, in reference to the change of colour in the blood, both venous and arterial, Dr. S. has frequently filled one glass with the black fluid taken from the heart, and another with the black vomit taken from the stomach—they were so similar that it was almost impossible to distinguish the one from the other. *Thirdly*, in violent continued fevers, the saline matter, like the fibrine, appears to be exhausted faster than it enters the circulation. The blood soon loses a great portion of its saline impregnation, and the entire of its saline taste, the cause of which, the author afterwards endeavours to shew, is owing to the loss or great diminution of the saline matters. *Fourthly*, the blood, though dissolved, was not putrid —the latter state being, indeed, incompatible with life. But dissolution he considers as the first step towards putrescency—and the cause rather than the effect of death; because he has seen the blood before death, so black and so thin, that it could scarcely be retained within the vessels—oozing from the tongue, the eyes, the skin, and other surfaces.

Conceiving, then, that this dissolved state of the blood was the cause of death, the great object of inquiry was to find out an agent capable of preventing this fatal change. In all climates, Dr. S. observed, the waters of the deep were preserved from putrefaction—and this preservation, he thinks, is probably owing to their saline impregnation. It is also well known that some of the saline medicines possess great antiseptic powers. Saline matters are invariably found in healthy blood, and as these were invariably lost or greatly diminished in the blood of fever patients, Dr. S. was naturally induced to try the effect of saline medicines, in preventing the bad symptoms of tropical fevers. The result of experience, he avers, was a conviction that those agents had, when used at a proper time, a specific effect in preventing the dissolution of the blood. In all the cases where they were timely and properly used, they prevented the fetor of the breath, the stoppage of the secretions, the yellow colour of the skin, the black vomit, and the other

fatal symptoms, so common in those cases where these medicines were not administered.

Our author observes that one common property of neutral salts is that of giving a rich arterial colour to venous blood. This property is common to them all, and the degree to which they possess it, is, perhaps, the best test of their purity as saline agents. To ascertain the effects of different agents on the blood, he made a number of experiments, in which it was observed—

1st. That all the acids give a dark colour to healthy blood, and in proportion to their strength, change it from red to black, as certainly as they change vegetable colours from blue to red. Even the vegetable acids so completely blackened the blood, that the addition of a little water converted the whole into a fluid exactly resembling the black vomit. *Secondly*, the pure alkalis have a similar effect with the acids, in changing the blood from red to black, though not in the same degree. *Thirdly*, the neutral salts immediately changed the venous blood from a dark modena red, to a bright arterial colour. Even those salts that contain a slight excess of alkali, the sub-carbonate of soda for example, immediately give to venous blood a beautiful bright arterial colour. The effects of these experiments are best seen when made on healthy blood. The agents ought first to be dissolved in a little soft water, and then well mixed with the warm blood, before it begins to coagulate.

Fourthly. When the neutral salts are mixed with the dark and dissolved blood that had been taken from the hearts of those who had died of yellow fever, even the black and dissolved fluid was instantly converted from a black to a bright arterial colour.

The nature of this paper (said Dr. S.) prevents me from entering minutely on the important effects which this saline impregnation produces in the vital fluid; but, in a work, which will soon be published, I shall endeavour to prove, first, that the blood owes its red colour to this saline impregnation. Black appears to be the natural colour of the colouring matter; for, when we take a clot of blood, and deprive it completely of its saline matter, by immersing it in fresh water, the colouring matter soon becomes so black, that even oxygen has no effect in changing its colour. But, when we immerse this black clot in an artificial serum made by dissolving some saline matter in water the black clot in this clear fluid assumes almost immediately a beautiful bright arterial colour. Secondly, that, to this saline the fibrin owes its fluidity: for it remains fluid only while mixed with the saline matter, and

becomes solid when the saline matter leaves it to unite with the serum. Thirdly, that the change of form which this saline matter undergoes, when the blood changes from arterial to venous, and from venous to arterial, alters its capacity for caloric, and gives it an influence in supporting the temperature of the system. The saline impregnation also adds to the stimulating quality of the blood, and assists, even in a high temperature, in adding to its powers of self preservation.

In the present state of our knowledge, both of pathology and animal chemistry, it is, Dr S. justly observed, best to rest simply on facts. Those which he has endeavoured to establish in his inquiry are partly the following:—1st. That, in violent continued fevers, even where proper means are used to protect the organs, by reducing the excitement, chemical changes often take place in the whole circulating mass; and, in these fevers, such changes are almost always the sole cause of the mortality. In proportion as the disease advances, the blood loses its solid part, and becomes thin. It loses its saline matter, and becomes both black and vapid. It loses its preservative power, and goes fast to decay. It loses its vitality, and soon becomes incapable of stimulating the heart or of supporting life. In the yellow fever, in the African typhus, in the plague, &c. the dissolution of the blood is a common cause of death. The typhus of cold climates is, comparatively speaking, a mild disease; but even in the common typhus, Dr. S. thinks that similar changes take place in the blood, though in a less degree. 2dly. In all cases of bad fevers, the loss of the saline or preservative power appears to be, in every instance, the chief cause of the entire dissolution of the vital fluid. 3dly. Where (says Dr. S.) proper means are used to protect the organs from the increased excitement, during the early stage of the disease, and after the excitement is sufficiently reduced, when proper nourishment is given and certain saline medicines are timely and judiciously used, the bad symptoms are generally prevented. When proper saline medicines are used, they do not fret the stomach, they act on the intestines as much as is necessary, they keep up all the secretions; particularly that of the kidneys, and enough is absorbed to enter the circulation and prevent the disolution of the blood, and preserve it until the fever abates, and all the danger is past. This I am warranted to state as a fact, inasmuch as the treatment was commenced in the West Indies, in 1827, and since then it has stood the test in several hundred cases of the West India fevers, where it has been tried both by myself and others, and with scarcely a single loss, when we were called to the patients

within the first twenty-four hours after the attack, and with very few deaths where we were called in previously to the commencement of the fatal symptoms. My friend Dr. George William Stedman, now of St. Thomas's, and others have adopted the same treatment, and the result in their practice has been similar to that which occurred in my own cases.

In August 1828, at a time when there was a good deal of sickness in the garrison at Trinidad, this practice was adopted in the military hospital of that island; that is to say, they bled freely, and used active purgatives in the commencement, to reduce the excitement, and afterwards the saline medicines were administered until the fever abated, and during the convalescence the quinine was given in large doses. In a communication which I received from Mr. Greatrex, of the Royals who at that time had charge of the hospital, he states "that the above system has been applied to three hundred and forty cases or thereabouts, including both the remitting and yellow fevers, admitted into the hospital after the fever had existed, variously from six to seventy two hours antecedently to an application to the hospital, with such success, that during the last seven months not a case has died." This document is dated about seven months after the commencement of this practice. Mr. Greatrex also states, that within that time three men died having the remitting fever, but they had also abscesses in the lungs, and purulent expectoration. As these three cases were complicated with extensive organic disease in the lungs, it is probable they would have been fatal under any treatment. But out of the three hundred and forty cases of essential fever, which had been treated in the manner described, there was not one death in the Royals from the time that this practice had been adopted; and I may add, that in the West Indies, Trinidad is generally considered as one of the most sickly islands.

It can be clearly proved, that in the West India fevers, those patients that are left entirely to themselves, have a much better chance of recovery than those who are treated with emetics, calomel or antimony, opium or acids, and that these remedies instead of being useful, add greatly to the suffering of the patient; they decidedly increase the very evils that they are meant to relieve, and add greatly to the mortality in hot climates.

Finally, Dr. S. considers it an error to consider fevers as entirely a disease of the solids—and still more so, to treat it merely with reference to the mere state of excitement. In respect to yellow fever, he

thinks it can only be treated with success, when we reduce, by active measures, the increased excitement, at the commencement, and then prevent, by proper means, those chemical changes in the blood, which are in reality, the sources of the diseased action in the solids, and the true cause of the mortality in these dreadful fevers. Convinced by numerous facts, Dr. S. adopted a mode of treatment widely different from that which he had formerly employed, and, in as far as it has yet gone, the use of the Rochelle salts, (tartarized soda) carbonate of soda, and other active saline medicines, at a proper period of the disease, has been attended with a train of success to which the mere Solidists can produce no parallel. He is convinced that, if this practice be generally adopted, the mortality from West India fever will be greatly lessened.

As we have no doubt that Dr. Steevens will pursue this interesting inquiry further, and lay the results of his observations before the profession in a more extended form, we shall abstain from any comments on the present occasion. We have laid a very full and faithful account of the paper before our readers, and leave them, for the present, to draw their own conclusions.—*Medico-chirurgical Review, July* 1830.

On the production of Worms in the human body. By J Scouler, M. D. &c. Professor of Natural History in the Andersonian University—A very ingenious paper on the above intricate subject is published in the May number of the Glasgow Medical Journal, from which we are tempted to make an extract. The formation of worms in the human body is one of the most difficult problems in Physiology—and is one of considerable interest even to the practical physician. We do not deem it necessary to go over the various hypotheses that have been framed, from that of "equivocal generation," maintained by the ancients, down to the revival of the same hypothesis by Blumenbach, Rudolphi, and other German naturalists It is a startling doctrine! We have the authority of Holy Writ that man himself is only a worm;—and if worms are produced by equivocal generation—or, in other words, by "fortuitous concourse of atoms," it is needless to suppose that the act of the Deity was necessary for man's first creation from the earth on which he crawls! With the hope of releasing us from this humiliating supposition, we gladly introduce to our readers the lucubrations of Dr. Scouler. After a critical analysis of the doctrines maintained by ancients and moderns on this subject, he thus developes his own ideas.

"We have now given a very brief outline of the theories by which the origin of worms has been attempted to be explained; but enough has been stated to prove that every one of them is utterly at variance with well known facts. One theory yet remains, founded on very different principles, and which demands our most candid consideration.

"Do worms originate from a spontaneous generation? Generation, or the production of a living being, consists, if we separate it from all accidental circumstances, in a separation of a portion of the parent animal, which is endowed with independent vitality. This portion is formed in the ovarium of the female before she has any intercourse with the male, and the principal effect of the spermatic fluid is to excite the dormant powers of the germ into action, and to modify its form. This is the essence of generation properly so called; but there is another way in which animals, or parts of animals, may be produced. If we cut off the leg, or extirpate the eye of a water newt, these parts are again restored after the interval of a few months, and only differ from those which had been destroyed in being a little smaller. The claws of lobsters and the rays of star fish are re-produced in the same manner. In the lower animals, this process reaches its maximum of development, and becomes a new mode of continuing the species. If we cut a polype into two portions, and the circumstances be favourable, each section becomes a perfect animal. Many worms, if divided in this manner, grow under the knife, and, if abundance of food be supplied, the division may be continued ad infinitum. A very common fresh water worm, the Planarea of naturalists, if well supplied with food, exhibits this process spontaneously. The animal begins to swell, and several fissures appear on its surface, and it gradually separates into several distinct portions, each of them becoming a perfect animal. Hence generation, by means of sexes, is not necessary for the continuation of animals, and, the law of Harvey, *omne vivum ex ovo*, must be changed into *omne vivum e vivo*. This mode of reproduction is by no means rare among the simpler animals; indeed it appears to be the only one by which the infusory animalcules are propagated.

"Having proved that generation, by means of two sexes, is not essential for the propagation of many animals, we are prepared to advance a step further, and maintain that entozoa may be produced by the animal which they inhabit. When we consider the extreme complexity of the organization of the more perfect animals, and vast variety of tissues which enter into the composition of their organs, we may grant

that it is not impossible that the vessels which erect such delicately constituted parts may form others of far greater simplicity. Thus, in many diseases, tumours and tubercles are formed in the organs of animals; and these new formations possess a structure as different from any of the regular tissues of the animal which produced them, as the texture of an entozoon does from that of the creature it inhabits. These tumours may be considered as possessing a degree of vitality peculiar to themselves, and only deriving their nourishment from the body to which they are attached. In the same manner, after rupture of the uterus, when fortunately for the parent she survives the accident, the fœtus becomes encysted, we have a phenomenon of the same kind, the fœtus retaining a portion of its vitality, and the requisite nourishment is derived from the vessels of the mother.

"In the instance of tumours and tubercles, already alluded to, their origin is doubtless to be accounted for from a deranged action of the assimilatory arteries, which is of two kinds: in the first they produce a formation which is analogous to the regular structure of some other part of the body, as ossifications and steatomatous tumours. When nutrition is still more deranged, parts are formed which possess no analogue whatever in the healthy state.

"Now, it is not improbable that many entozoa owe their orgin to a process similar to that we have described. Worms are always a consequence of a diseased state, and when they produce bad symptoms, it is from some accidental circumstance depending on their size, numbers, movements, or from the importance of the organ in which they are found. So that there is a diathesis peculiar to most organic diseases, there is also a verminous diathesis. In support of this, it may also be added, that worms differ from all other animals in being intimately dependant on the organization which they inhabit for the continuation of their existence; and, in this respect, the parallel between encysted worms and the encysted fœtus, already mentioned, is perfect in every point of view. As the mode of nutrition in every animal is, to a certain degree, *sui generis*, so the worms which they produce are also peculiar.

"To express clearly the manner in which worms are supposed to be formed, and to prevent mistakes, we will condense what has been said in a few words. It is not maintained that entozoa are produced from the fluids found in the intestinal tube or in the tissue of organs; but that their germs are formed by some aberration of nutrition, in a manner similar to that in which other new formations originate, and that this

germ becomes endowed with an independent vitality, and is evolved into a worm.

"The consideration of hydatids affords us a proof of the manner in which entozoa are formed. These cysts are of two kinds, those which are mere serous vesicles, and those which are true entozoa; but the transition, from the one to the other is very gradual. We first observe a serous cyst, smooth, polished within, and containing a limpid fluid, and exteriorly fibrous. The next kind, the Acephalocysts of Laennec, consists of an internal fibrous capsule, containing a cyst, which is very thin and transparent, filled with a fluid, but destitute of a head or any distinct organs; these are intermediate between the cysts of the first kind and the true entozoa; so that some writers include them among the common hydatids and others among the entozoa. We have next the Echinococcus, which is included in a fibrous cyst like the preceding ones, but contains in its interior many little bodies, which are acknowledged by all writers to be true worms. From these the transition is easy to be cysticerci, with a head like that of a tape worm, attached to a transparent cyst, included, as usual, in a fibrous one. Lastly, we have often found round worms included in such cysts. These facts afford a strong support to the theory we have adopted.

"There are several objections which may be urged against this theory, which it is necessary to notice. As many entozoa have distinct organs of generation, and some of them have individuals of both sexes, and possess a very complicated organization, it has, with great propriety been urged, that there is no need for seeking any other way of accounting for their propagation than the usual one, and that it is contrary to every analogy to attribute their formation to the process we have described.

"To this remark it may be replied, that it has already been demonstrated that the ova of entozoa cannot travel through the tortuous paths of the circulatory vessels; and, further, many worms, as the hydatids for example, possess no genital apparatus. Nor is it to be ascertained that, if the theory we have advanced be correct, organs of generation are of no use; for, if a worm be once formed, it then propagates its species in the usual manner. A still greater alleviation of this objection is derived from the fact, that many animals, endowed with organs of generation, are frequently propagated by a process totally distinct from it, namely, by a division of their bodies.

"Another objection is, that if entozoa were produced in the manner described, we should have nothing regular and determined in their

forms, but that new animals of monstrous forms and confused organization would be daily arising.

"Nature, however, leaves nothing vague and undetermined in her works; and the production of entozoa is regulated by fixed and unchanging laws. The same modification in the nutrition of an organ, which produced the first intestinal worm, still continues to operate, and consequently the same animal is formed. We might as well expect to have new diseases springing up every day, and that laws, which regulate the formation of a cancer or an exostosis, should be different in every individual. Nature is always uniform in her operations, and even her most irregular productions are governed by regular laws. Even monsters, which, for a long time, were supposed to be the production of some fortuitous concourse of circumstances, can now be classified and described, and the laws of their formation ascertained with as much precision as those which preside at the origin of a regularly organized individual.

"A third objection which may be urged is, that according to this theory, if worms are produced in the manner it supposes, we should have some regular apparatus of a temporary nature, at least, set apart for their production.

"We may reply to this argument, that there are many parts formed in the bodies of animals differing completely from those which are found while it is in a healthy state, and yet we see no apparatus by which they are called into being. We see no apparatus for the production of encysted tumours, or for the formation of earthy matter in the valves of the heart. In these instances, as in the origin of entozoa, we have merely to attribute all these phenomena to the abberrations of nutrition, which constitutes the essence of every disease, and the chief cause of every organic change."

We beg to return our thanks to Dr. Scouler for the pleasure we have derived from a perusal of his interesting paper.—*Ibid.*

On Amaurosis. By James J. Knox, M. D.—Amaurosis from Suppressed Catamenia.—(Glasgow Med. Jour. May 1830.)—A young robust woman applied on account of the diminution of her vision. Saw objects dimly, and covered with motes; pupils were enlarged, and sluggish, but jet black; eyebrows, also, were swollen, and felt heavy. Stated that the disease came on a month previous to application, after a sudden suppression of the catamenia. An emetic with emmenagogue

medicines were prescribed. Three days after application, a great change to the better, saw objects clearer and free from motes; eyelids, however, still appear swollen, in consequence of which she was bled to twelve ounces, and in a few days dismissed cured.—*Ibid.*

"*Amaurosis from sympathy.*—Amaurosis is also sometimes produced from the retina sympathizing with some other part of the body. This species might with propriety be called amaurosis sympathetica, vel gastrica.

"The remote sympathies that exist between the different parts of the body, are often truly astonishing, and with none are these more strikingly exemplified, than with the abdominal viscera. Hence amaurotic affections are often produced by gastric impurities, and derangements of the primæ viæ. Many examples are on record of amaurosis being produced by a disordered state of the alimentary canal, as well as after eating a hearty meal. The most marked example however, of amaurosis being produced from sympathy, is that which is often met with in children troubled with worms. I have only once met with a decided case of this kind. The patient, a child seven years of age, was brought to me in August 1828. The case is entered in my eye case book as corneitis of left eye, with sympathetic amaurosis of both from worms. Small doses of calomel and scammony were prescribed with the happiest effects, a great number of ascarides being discharged. In proportion as these were removed the amaurotic symptoms disappeared. The following in general will be found to be the symptoms, when amaurosis is produced by worms. Eyeballs natural in appearance, pupils very much dilated and sluggish, but jet black, belly generally tumid, or enlarged, appetite voracious, with picking at the nose.

"One of the most remarkable, and at the same time distinguished cases of amaurosis from sympathy that I have heared of, occurred lately. The patient, a young lady, had a needle in the ribbon around her waist, which accidentally run into the hand near the wrist and broke The family surgeon had to be sent for to remove it. Severe pain was experienced at the time, and continued for sometime afterwards. She became on the following day (I believe) amaurotic of one eye, and of the other also in a few days after.

"This and the first species of amaurosis are the only instances, as far as I have observed, that are ever remittent, intermittent, or periodical. For those cases which depend on the greater clearness of one

day, or part of it, over that of any other, are not to be considered either as remittent, or periodical, as they obviously depend on the degree of light presented to the eye, and not on any variation of the powers of vision. They also admit of being oftener cured than any of the other species, because they do not depend on any organic change of the eye, or its appendages, and hence it is that the pupil in them is always jet black.

"*Treatment.* It is in this species of amaurosis that the emeto-purgative plan of Scarpa and Richter will be found most serviceable. If, as sometimes happens, the amaurosis should have succeeded a hearty meal, and there is reason to suppose that the disease is produced by it, an emetic should be administered, and repeated at proper intervals. The bowels ought also to be kept freely open, with pills composed of calomel and colocynth, or any other strong purgative.

"When worms are the supposed cause of the affection, medicines calculated to remove these should be administered. A combination of calomel and scammony I have found a most valuable anthelmintic. Turpentine enemata are also highly beneficial.

"The treatment of sympathetic amaurosis from wounds, must be regulated by existing symptoms. The free division of the part as recommended by Beer and others in amaurosis consequent from wounds of the superciliary nerves should not be lost sight of, as in some cases of the above description, it has been found serviceable. Strict attention must also be paid to the state of the alimentary canal."—*Ibid.*

Cursory Remarks on the Presence of Worms in the Large Bowels.—Case in which Lumbrici passed from an opening at the Umbilicus. By James Annesley, Esq.—(On the Diseases of India.)—

"Intestinal worms are very frequently met with between the tropics. Among Europeans they are generally the consequence of torpid bowels, or accumulations of morbid secretions in the alimentary canal. Ascarides, Lumbrici, and Tæniæ are the most frequently observed. Among the natives there are scarcely any free from these parasitical animals.

"It will generally be found amongst the natives of warm climates, and among those Europeans who have been much weakened by their residence in them, that the secretions, which form the principal part of the fæcal discharge, are seldom thrown off from the mucous surface of the large bowels in so quick a manner as in the robust individual

who enjoys an energetic state of the circulation and of all the organic functions; and being thus retained, they form at least the soil in which worms are reared, whatever may be the primary source from which these creatures proceed. Hence the importance of endeavouring to prevent the retention of morbid secretions and fæcal matters, and to impart energy to the digestive functions generally.

"The possibility of worms perforating the parietes of the intestines has been argued for by some pathologists, and denied by others. Without pretending to decide the question, we shall adduce the particulars of a case which came before us, in which the parietes of the bowel must have been perforated by them; but whether the perforation was effected previous to inflammation and ulceration having been excited in the part of the intestine in which they were lodged, or subsequently to that event, is a point which cannot be determined, from the imperfect history of the case, to which we were called only in its last stages.

August 5th, 1820.—J. W———, four years of age, according to the account furnished by his father, began to complain, about three months since, of swelling and hardness about the umbilicus, with pain on pressure. Opening medicines were prescribed by the medical man in attendance; and afterwards, finding that the tumor did not subside from their operation, poultices were applied. From the middle of April, the time at which the hardness was first detected, until the end of July, it gradually increased. The bowels, however, were always regular, even without the assistance of medicine, and the appetite was unimpaired. During July the swelling had increased considerably, was fluctuating, and slightly inflamed. The child's temper became irritable, and considerable symptomatic fever, with loss of appetite and cerebral irritation, supervened. Animal food was now abstained from, and saline diaphoretics and laxatives were given. On the first of August the abscess broke through two openings in the umbilicus, and discharged a great quantity of thick offensive matter. The usual dressings were applied. On the 2d, about a pint of yellowish watery fluid was discharged, with some thick offensive matter, similar to that which passed on the preceding day; and as a substance appeared to protrude through the aperture, which the father of the child fancied was the bowel itself, he became alarmed, and sent for us. We immediately drew from the opening two large lumbrici. This was the first time of our seeing the case. After this the child lived several days; the

fæces, with eight or nine large lumbrici, passed through the opening at the umbilicus, and very little by the anus, during this period.

"*On examination*, the lower part of the ilium was found obstructed, its convolutions agglutinated together, and its canal, in parts, constricted to the size of a goose-quill. It presented no marks of recent inflammation, and was of a pale colour, both externally and internally. The agglutinated mass of small intestines adhered also to the abdominal parietes, around the umbilicus; and one of the most superficial convolutions of the intestine had an ulcerated opening through it, communicating with the external aperture at the umbilicus. The other abdominal viscera were natural in appearance."

In respect to the treatment, Mr. Annesley has not any thing new to offer. After proper purgation has been pursued to clear away the nidus of the worms, he was in the habit of prescribing enemata of castor oil and spirit of turpentine with great advantage. After this, tonics were ordered. In cases of tænia, the oil of turpentine was advantageously given by the mouth, as also the bark of the pomegranate-tree.

Ibid.

Madame Boivin on a New Cause of Miscarriage.—She conceives that a common cause of miscarriage is pre-existing organic derangement of the uterine appendages,—of such a kind, that although in the unimpregnanted state the health does not necessarily suffer, yet after impregnation the uterus cannot expand itself freely as the fœtus grows, without the parts being subjected to painful dragging and consequent irritation. The most frequent organic derangement of this nature is simple adhesion of the uterine appendages to the uterus itself, to adjacent organs, or to the peritonæum of the pelvis; and another variety of disease to which the authoress imputes the same effects, is an indurated or tuberculated condition of the ligaments of the uterus. In any of these states of the organs of generation, as soon as the uterus has attained a certain size, corresponding in general with the fourth month or thereabouts, it cannot increase any farther, so as to rise out of the pelvis, on account of the rigidity of its ligaments, or the adhesion of its appendages to the uterus, the pelvic viscera, or the parietes; it is therefore subjected to more and more violent irritation, till at length it is stimulated to contract, and throws off the ovum contained in it.

This view of the cause of miscarriage in such cases is founded partly on the dissection after death of individuals who died soon after miscarriage, and in whom it was quite evident that the diseased state of the

uterine organs must have existed long before,—and partly on instances of recovery from frequent miscarriages, where the diseased state of the uterine appendages was detected by an examination of the living body. The characters remarked during life are, according to the authoress, sufficiently precise. They are resistance of the uterus to an impulse communicated by the finger applied on the os uteri, unnatural immobility of the uterus when an attempt is made to move it by means of the finger in the rectum, sometimes the obvious discovery of organic disease on an examination by the rectum, great tendency to constipation, and, lastly, in a few cases unusual proximity of the os uteri to the perinæum, conjoined with the character already mentioned, the impossibility of pushing it upwards.

The following cases will serve to exemplify the affection which *Madame Boivin* describes. The first is a case of death from miscarriage in the fifth month of pregnancy, the cause of death being united inflammation of the peritonæum and suppurated tubercles in the lungs.

A widow, 27 years of age, tall in stature, moderately plump, and of a lymphatico-nervous temperament, was delivered of her first child with the forceps, after being sixty hours in labour; and had afterwards two children born at the full time, and naturally. She was all this time poorly lodged, her living was indifferent, and her manner of life was very sedentary. In February 1826, she was attacked apparently with violent peripneumony; and after this had existed some days, she miscarried in the fifth month of her fourth pregnancy. Three days afterwards she was admitted into the Maison Royale de Sante; but the affection of the chest made rapid progress, and proved fatal in six days more.—On dissection, the pleura was found every where adhering; the right lung contained a cavity as big as the fist filled with purulent matter; and the left lung was studded with tubercles of various sizes, some of which had passed to suppuration. The stomach and intestines were red, the peritonæum redder and thicker than natural, and its cavity contained some yellowish serum. The broad ligaments, the Fallopian tubes and the ovaries were agglomerated and attached to the posterior surface of the uterus by adhesions of such strength, as to require the scalpel for their destruction. In the substance of the agglomerated textures were remarked numerous tubercles between the size of a millet-seed, and that of a pea, and quite analogous to those found in the left lung. "It is evident," adds the authoress, "that with such a state of the uterus and its appendages, this organ could not develope itself

but with great difficulty. Abortion would undoubtedly have taken place even though the chest had been sound. For the ligaments could not have yielded to the expansion of the organ to which they were firmly attached; and their resistance becoming the source of uterine irritation, contractions of the womb and the expulsion of the fœtus were the inevitable consequences." *P.* 3.

Several other instances of the same kind are related. The only other case we shall notice particularly is one of those where the tying down of the uterus within the pelvis was recognized during life, and considered to be the cause of the frequent miscarriages which the patient had experienced.

Madame E., 26 years of age, stout in constitution, and of a lymphatico-sanguine temperament, had her first menstrual discharge at the early age of seven, and after her tenth year had it regularly. At thirteen she had an attack of menorrhagia; at fourteen she was married; in her sixteenth year she had her first child at the full time, and safely; and in her eighteenth she sustained a fracture of the left thigh-bone. After her twenty-third year she had three miscarriages in a year and a quarter, at the fourth, second, and fifth months. Then the catamenia became very irregular; and she had another violent attack of menorrhagia, followed by acute pain in the right iliac region, much vomiting and considerable fever; for which leeches on the belly, poultices, and anodyne injections were required. In two months more she had a similar attack, when the state of the genital organs was for the first time carefully examined. The uterus was found so low as almost to rest on the perinæum; its cervix was larger than natural, and tender; the *os uteri* was open, and surrounded by a circular collar formed by a fold of the upper end of the vagina; and the uterus could not be forced back but with great difficulty, nor without occasioning acute pain in the left haunch and groin. The same treatment was followed as in the former attack; and in order to combat the obstinate constipation to which she had been long liable, emollient and purgative clysters were used, though for some time unsuccessfully. A temporary improvement was thus procured; but the symptoms recurred again with violence, and were not permanently removed till she underwent a mercurial course. In this case every circumstance announced an organic derangement in the internal organs of generation of very old standing. This appeared from the premature menstruation, her liability to menorrhagia, and her frequent miscarriages. The symptoms of enteritis might also suggest the probability of adhesions between the uterus and intestines.—*P.* 44.

In cases of the affection here described, the cause of the organic derangement of the organs of generation has sometimes obviously been inflammation of the pelvis; but in the greater number it has arisen without any distinct symptom to mark the source or time of its formation. Cases of the latter description have appeared to the authoress to occur chiefly among females with dark hair, blue eyes, and particularly bluish sclerotica, and who have led in their youth a sedentary life, with habitual neglect of the bowels. Indeed she is inclined to trace the disease in a great measure to habitual constipation. She thinks a course of mercury has sometimes proved effectual in removing the organic disease, or at all events in rendering the female capable of carrying her child to the full time; and conjectures that in the instances where Dr. Granville thought he succeeded in removing by mercury a tendency to miscarriage, the tendency must have been owing to the disease now described.—*Edinburgh Med. and Surg. Jour Oct.* 1830.

Remarks on the Treatment of Amaurosis by Strychnine, with several successful Cases. By Thomas Shortt, M. D., F. R. S. E. Fellow of the Royal College of Physicians, Edinburgh, one of the Ordinary Physicians to the Royal Infirmary and Fever Hospital, &c.—Having been induced, by the perusal of several successful cases of paralysis, published in the Glasgow Medical and Surgical Journal of May 1829, to try the effects of strychnine introduced into the system through blistered surfaces, it occurred to me, on examining a patient sent by Mr. Liston from the surgical wards of the hospital, and who laboured under amaurosis, apparently owing to paralysis of the retina, that this substance might be beneficially employed in relieving blindness from that cause. As in this case almost every other remedy had failed, I lost no time in putting the patient under its influence. The effects, as detailed in the case of Hamilton, No. 1 of this series, were so satisfactory that I felt warranted in giving the remedy further trial. The result of this will be seen by the cases which follow his. It was not my intention to have laid them before the public until I not only had decided in my own mind as to the efficacy of this medicine, but also was enabled, from farther experience, both to give some specific directions in regard to its employment, and, what is of much more importance, to point out the symptoms of those cases of the disease in which its use promises success. Some of the cases, however, which had

been thus treated by me, having been published without my consent, I have been urged by several professional friends to submit, without further delay, even an imperfect sketch to the profession. I own I do so reluctantly, as I am satisfied that much is yet to be learned regarding the use of strychnine; but not without hope that it may prove of service in exciting to a more extensive trial of the remedy than my situation, unconnected at present with any of the public eye institutions, can permit me to make. Having formerly had charge of the Ophthalmic Hospital in Egypt, serving with the army under Lieutenant-General Mackenzie Fraser in 1807, afterwards having been entrusted with the same duty in Sicily, and lastly with the entire charge of an experimental hospital on a large scale at Rhametta in that island, expressly formed for the treatment of chronic ophthalmia and its consequences, gave me ample opportunities of witnessing such diseases under almost every form, and at the same time of often regretting their unsuccessful issue, especially in amaurosis. Such experience is calculated to secure me against the error of attributing to one means only the power of relieving this disease or any other affection; while, in the absence of any thing more satisfactory respecting the management of a very untractable affection, it may entitle me to some confidence in offering the present practical remarks. Moreover, as perhaps the first person who employed strychnine in amaurosis, and as having used it more extensively than most other individuals, I imagine my observations can hardly be deemed either intrusive or unimportant, however imperfect or inconclusive future inquirers may prove them to be.

I consider strychnine to act purely as a stimulant either of the nervous matter of the nerves, or of their capillary system; and, therefore, to be successfully employed only in amaurotic cases, which depend on paralysis of the optic nerve and retina, or in cases of congestive amaurosis from pressure on these parts, with a loaded and inactive state of the minute vessels of the neurilematic envelope.

These affections are often ascertained with difficulty, and, therefore, cases may be viewed as such, though arising from structural derangements of the interior of the organ of vision. In these, as in others arising from severe or long continued internal inflammation and other causes, strychnine, I apprehend, can be of no possible service. The same thing may be said of amaurosis depending on clots of blood, or tumours which press upon the optic nerves. Where again, as is frequently the case, the disease is owing to the absorption of the medullary portion of the

nerve, it must be allowed that nothing can be expected from it or any other medicine. In fine, in my opinion, it is only in cases of paralysis of the optic nerve, and those arising from congestion, that strychnine can be advantageously employed; and I think the following examples, and those already published by Mr. Liston, Mr. Guthrie, and others, must decidedly prove its efficacy in them. It is not immaterial to observe, that in such cases its beneficial operation is considerably aided by the previous use of mercury, possibly either by exciting the nervous system, by rousing the energy of the capillaries, or by increasing the powers of absorption, or by all three conjointly. Thus, in No. 5, strychnine failed until mercury had been employed; but on its application, and then only, vision was improved; and in cases No. 7 and 9, its decided effects followed the use of that medicine.

I cannot positively say whether the effects of strychnine are lasting, but I believe it in most cases to be so, if properly used. I may safely assert also, that in no case in which I have tried it has vision been injured, where the disease existed in one eye only, or where sight was not entirely destroyed. In no case did any constitutional evils arise from its use. I am inclined also to believe, from one or two instances, that it was beneficial in removing opacities of the cornea, probably by its highly stimulating property occasioning rapid absorption.

In delicate persons, or where the system is affected by mercury, I ought to add, the strychnine should be commenced in small doses, *e. g.* not exceeding a quarter of a grain, and increased daily until it produces sensible effects on the constitution, such as headach, pricking pains over the body or tremors, when it should be discontinued, and, on resuming it, the dose should always be considerably reduced.

Where unpleasant symptoms arise, I can suppose that camphor in large doses, or an opiate enema, suited in strength to the violence of the symptoms and the constitution of the patient, or, as recommended by M. Lembert, the application of morphine in small doses, sprinkled on the blistered surfaces, will be found to give relief; but in no instance have I judged it necessary to employ any of them.

Several of the cases here detailed were attacked with erysipelas, which strychnine seems prone to occasion; but they were invariably relieved by simply rubbing about a drachm of mild ointment or cold cream over the diseased surface every four hours.

Mr. Fife, surgeon of the Eye Infirmary at Newcastle, has employed strychnine internally in several amaurotic cases, accounts of which he

has had the kindness to send to me. Two are now published with his permission. All the cases relieved by me, I ought to remark, had some remains of sight before this remedy was employed; and it may not be uninteresting to add, that in a case of glaucoma, attended with deafness, where strychnine was used, the latter affection was relieved, but without any improvement in vision, as was also the result in one of Mr. Fife's cases.—*Ibid.*

(Here follow nine cases in all of which the strychnine applied locally to small blistered surfaces on the temples, afforded more or less relief—in some completely restoring vision which had been nearly lost. The medicine was ordinarily applied in minute portions, 1-4 of a grain at a time repeated daily, and gradually increased to as much as 5 and 6 grains.)

On Oleum Terebinthinæ as a remedy for Salivation. By E. Geddings, M. D. Lecturer on Anatomy and Surgery, Charleston, S. C.—It has long been matter of regret, that the good effects of mercury are so far counterbalanced by the sufferings attendant upon its operation upon the constitution, as to deter many, in a measure, from using it, and to induce some to submit quietly to the sufferings of their malady, rather than incur the still greater sufferings inflicted by a mercurial salivation. It often happens, moreover, even under the most prudent and circumspect employment of mercury, that certain individuals, from peculiar idiosyncrasy, or some other cause, become severely salivated. It must then be confessed that a remedy calculated to mitigate the sufferings thus accidentally induced, is a desideratum, and that any thing which holds out the slightest promise of benefit is deserving of an impartial and candid trial. It is under these circumstances, that we venture to offer the results of our own experience upon this subject, trusting that the remedy which we are about to propose may prove as successful in the hands of others as it has in our own. For several years past, we had been completely baffled in all our efforts to relieve a mercurial salivation. We had tried all the usual remedies without any very striking results. Sulphur had always disappointed our expectations. Emetics had sometimes afforded relief, but they can only be employed in a few cases. Cold, recommended by some, is not always safe. Opium had only served to allay pain. The ordinary astringent gargles had not afforded much relief. Porter had occasionally succeeded. The Rhus glabrum recommended by Dr. Fahnestock we have never

tried. It was not until the winter of 1828 and 29, that we became acquainted with the efficacy of Ol. Terebinth. in the treatment of this distressing affection. We were attending a young gentleman of the medical class, who became severely salivated from a small dose of calomel. A highly intelligent young gentleman, Mr. J. E. Pierson, at that time one of our pupils, now a respectable physician of Fairfield District, proposed the turpentine, which was accordingly employed, and afforded speedy relief. Since that time we have used it extensively in both hospital and private practice, with the most satisfactory results. It has also been much employed by several of our medical friends, whose testimony in its favour, we are happy to state, fully substantiates our good opinion. We usually direct two drachms of turpentine to eight ounces of gum arabic mucilage, with which the patient is required to gargle frequently in the course of the day. We have, however, in some cases, employed the undiluted turpentine with the same happy effects. It usually occasions, at first, considerable smarting, which, however, is of only temporary duration, and becomes less and less at each successive application. Indeed, in many cases, the smarting is succeeded in a short time by a soothing impression, similar to that which attends the use of the turpentine in cases of burns and scalds.

We will say nothing of the modus operandi of the medicine in cases of salivation, but will merely recommend a fair and impartial trial of it as a remedy for that distressing affection.—*Amer. Jour. Med. Sciences, Nov.* 1830.

OBITUARY.

Died in this place on the last of September 1830 Robert Best, M. D. aged about 45 years. Dr. B. was among the most respectable physicians of Lexington, and formerly assistant lecturer on Chemistry in the medical Department of Transylvania University.

———in Lexington during the past autumn, of pulmonary consumption, Lawson H. Caldwell M. D. of North Carolina.

———in Troy, Missouri, in September last, of bilious fever, Dabney Carr M. D. formerly of Fayette County, Kentucky.

INDEX TO VOL. III.

Asylum Lunatic of Kentucky, account of - Page 79
Absorbent System and its Functions, Essay on 94. 185. 331. 520
Address Valedictory, *Caldwell's* - - 309
Acid, Prussic, antidote to - - - 148
——— ——— *Savage* on the - - - 157
Abortion, on perpetual vesication in prevention of - 149
Absorption, Venous - - - 201
Address, Annual before the Medical Society of the State of New York—1830 - - - 219
Autumnal Fever, *Payne* on - - 218
Arsenic, poisoning with - - 239
——— discovered in a body seven years after interment 431
Awl on the the Effects of Tobacco in Hernia, - 418
Amaurosis from Suppressed Catamenia, - 581
——— from Sympathy, - - 582
——— on the Treatment of, by Dr. *Shortt* - 588
Annesley's remarks on worms - - 583
Blisters, their use in Typhus Fever, - - 9
Bark and Wine, on their use in Typhus Fever, - 11
Belden on Vesication in the prevention of habitual abortion, 149
Brown, Dr. Samuel, obituary notice of - 152
Bone in the Bladder a cause of Calculi, - 211
Beck's Annual Address before the Medical Society of the State of New York, - - 219
Bandage, Cases illustrating its utility - 263. 569. 570
Bolling's case of Remittent Fever, - 266
Bone lodged in the trachea of a child, - - 296
Blood, observations on the - - - 572
Boivin Madame on a new cause of Miscarriage - 585
Contagion specific as the cause of Typhus fever, - 29
Caldwell's Thoughts on Generation, - - - 47
——— Valedictory Address, - - 310
——— Essay for the Boylston prize Question for the year 1830, 458
Cellular Tissue, - - - - 116
Consumption, Hereditary case of, cured, - - 128
Combustion Spontaneous, *Short's* case of - - 143
Coons on Congestive bilious fever, - - - 166
Cross's Essay on the Absorbent System and its Functions - - } 94. 185. 331. 520
Campbell's Cases of Lithotomy - - 211
Cunningham's Case of fracture, &c. - 264
Cloud's case of labour, - - 271
Columbo of the Materia Medica, - 277
Cataracts alternating with Diabetes, - - 282
Climate and Diseases of Tennessee, - 364
Cooke's Essay on Fever, - - 389
——— ——— on the origin of the Plague, - 553

Cerbera Tanghin, Poisonous properties of - 420
Catamenia Suppressed, causing Amaurosis, - 581
Diet and Drinks proper in Typhus Fever, - 9
Dermoid Tissue, - - - - 123
Dissertation, Inaugural *Nelson's*, - - 232
Dyspepsia, *Lyne's* case of - - 269
Diabetes alternating with Cataracts, - - 282
Decay of the body, on the mode of determining by, how long it has been in water, - - 428
Encephaloid Tissue - - 112
Emetic Tartar, on its use in certain states, - 127
Extirpation of the Uterus successful - 146
Emetics in Mercurial Salivation - - 232
Epilepsy, case of, resulting in insanity, cured, - 410
Elbow-joint, Gun-shot wound of the - - 570
Fever, Typhus, *Magill's* Essay on - - 9
—— ——. causes of - - 21
Ficklin, Dr. William A. Obituary notice of - 152
Fever, congestive bilious, *Coons* on - - 166
—— Autumnal, on its remote and exciting causes, - 248
—— Essay on - - - 389
Fletcher's case of bilious fever, - - 273
Generation, *Caldwell's* Thoughts on, - 47
Graduates of the Medical Department of Transylvania University for 1830, - - 296
————Valedictory address to - - 309
Gun-shot wound of the elbow-joint - 570
Geddings on oil of Turpentine in Salivation - 591
Hereditary Consumption, case of cured - 128
Hæmatemesis cured by Purgatives, Case of - 137
Harris's case of Hæmatemesis - - 137
Hydrophobia, Singular case of - 148
Hæmorrhage after extraction of a tooth - - 150
————Remarkable case of - - 260
Head, *Rogers's* case of injury of - - 284
History of the Climate and Diseases of Tennessee - 364
Hernia, on the Effects of Tobacco in - 416
———— on the use of Spirit of Turpentine in - 425
Hayward's case of Paralysis of the kidneys - 422
Hamilton's remarks on the use of Spirit of Turpentine in Hernia 425
Hardy's case of Laryngotomy - - 267
Inaugural Dissertation, *Nelson's* - 232
Knox on Amaurosis - - 581
Lunatic Asylum of Kentucky, account of - 79
Lucas on the use of Tartar Emetic in certain States of the System 126
Lithotomy, *Toland's* case of - - 139
———— *Campbell's* cases of - - 210

Lewis's case of Hæmorrhage - - 261
Laryngotomy, *Hardy's* case of - - 267
Lyne's case of Dyspepsia - - 269
Lee on inflammation of the veins of the uterus - 414
Lumbrici, case of their passing through an opening in the umbilicus - - 583
Magill's Prize Essay on Typhus Fever - 9
Muscular Tissue - - 117
Mucous Tissue - - 122
Miscellaneous Intelligence - 146. 276. 414. 572
Mercurial Salivation, Emetics in - - 232
Medical Society of the state of Tennessee - 439
——— Lectures in Transylvania University - 455
Miasms febrile, *Caldwell* on - 458
Miscarriage, on a new cause of - - 585
Nervous Tissue - - 114
Nelson's Inaugural Dissertation - - 232
Osseous Tissue - - 125
Obituary notice of *Dr. Samuel Brown* - 151
——— ——— *Mr. Patrick C. Snoddy* - 152
——— ——— *Dr. William A. Ficklin* - 152
——— ——— *Dr Stephens R. Beattie* . 296
Opium, poisoning with - - 292
Observations on the Blood - - 572
Oil of Turpentine a remedy for Salivation - 591
Origin of the Plague, *Cooke's* Essay on - 553
Pulmonary Tissue - - 121
Placenta retained, *Tracy's* case of - 141
Prussic acid, antidote to - - 148
——— — *Savage* on the - - - 157
Payne on the remote and exciting causes of Autumnal fever 248
Placenta delivered before the fœtus, - - 271
Paralysis ascending, curious case of - 280
Poisoning with Arsenic - - 289
——— with Opium - - 292
Peck on the Sanative influence of a Southern residence on Northern patients - - 404
Paralysis, case of, succeeding to Fever, - 411
Phlegmasia dolens, cases of utility of the Bandage in 414
——— ——— *Lee's* observations on - 569
Poisonous properties of the Cerbera Tanghin - 420
Paruria inops, *Hayward's* cases of - 422. 424
——— ——— *Short's* case of - - 425
Paralysis of the kidneys - - 422. 425
Pharmacy, importance of the Science of - 432
Prize Essay of the Med. and Chirur. society of Maryland, 453
Plague, *Cooke's* Essay on the origin of - 553

Recamier's case of Extirpation of the Uterus - 146
Rodgers's case of injury of the head - 284
Raines's case of Paralysis - - 411
Specific Contagion as the cause of Typhus fever - 29
Spontaneous Combustion, case of - - 143
Short's case of Spontaneous combustion, - 143
——— — of Paralysis of the Kidneys - 424
Syphilis, cases of, treated in the Hospital Val de Grace (Paris) 147
Snoddy, Patrick C. Obituary notice of - 152
Savage on the Prussic acid - - 157
Salivation, Mercurial, Emetics in - 232
——— ——— on oil of Turpentine as a remedy for 591
Southern residence, its sanative influence on Northern patients 405
Swan's case of Gun-shot wound - 570
Steeven's observations on the Blood - 572
Scouler on the production of worms in the human body 577
Sympathy, Amaurosis from - 582
Saline Medicines, on their utility in Fever - 673
Strychnine, on the treatment of Amaurosis by - 588
Shortt on the treatment of Amaurosis by Strychnine 588
Typhus Fever, *Magill's* Prize Essay on - 9
——— ——— of specific Contagion as a cause of - 29
Thoughts on Generation - - 47
Theobald's account of the Lunatic Asylum of Kentucky 79
Tartar Emetic, its use in certain states - 126
Tolana's case of Lithotomy - 139
Tracey's case of retained Placenta - 141
Tyrell's remarks on *Rogers's* case of injury of the Head 286
Transylvania University, Medical Graduates of - 297
Tennessee, on the Climate and Diseases of - 364
——— Transactions of Medical Society of - 439
Tobacco externally applied in Hernia - 418
Turpentine, Spirit of, in Hernia - 425
Terebinthinæ Oleum, on its use in Salivation - 591
Uterus, successful extirpation of 146
——— Inflammation of the veins of 415
Umbilicus, case of Lumbrici passing through the 583
Valedictory Address on some of the Duties and Qualifications of a Physician 309
Vesication in prevention of habitual abortion . 149
Vaccine Institution of Baltimore . . 454
——— ——— Philadelphia . . 454
Worthington's case of Epilepsy 410
Water, on the mode of determining by the decay of the body how long it has been in 426
Worms, on the production of, in the human body 577

Lewis's case of Hydrocephalus
Laryngotomy, Morely's case of 217
Lepe's case of Dyspepsia 219
Lee on inflammation of the veins of the spinal
Lumbrics, case of their passing through an opening in the
Magill's Prize Essay on Typhus Fever 1
Medical Times 18
Medical Times 22
Miscellaneous Intelligence 196, 227, 414, 472
Mercurial Salivation, Essays on 212
Medical Society of the state of Tennessee 43
—— Lectures in Transylvania University
Mission Medic, Caldwell's on
Miscarriage, on a new cause of
Nervous Tissue 21
Nelson's Surgical Observations 118
Opium Trade
Obituary notices of *Dr. Samuel Brown*
—— —— *Dr. Patrick C. Smedley*
—— —— *Dr. William A. Fellow*
—— —— *Dr Stephens R. Beasley*
Opium, poisoning with
Observations on the Blood
Oil of Turpentine remedy for Salivation
Origin of the Pupil, Guide's Essay on
Pulmonary Tissue
Phosphate ordinaire, Tonge's case of
Prussic acid, antidote to
—— —— *Essays on the*
Papers on the venous and spinning cancers of Antimonial Wine
Operations followed below the fistula
Analysis, spinning, cancerous case of
Chemistry with Anatomy
—— with Opium
Pail, on the Prussian ultimatum, a Necessary medicine in Cholera
Pus, case of, accumulating in Pleura
Pleurisy, case of, accompanying the Scarlatina
—— —— *Lee's* observations on
Pleurisy, case of, in Cerebral Tuberculous
Pneumonia, Bennett's cases of
—— —— *Hare's* case of